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A RESONANCE METHOD OF MEASURING ENERGY DISSIPATED IN CONDENSERS.

By Edward B. Rosa and Arthur W. Smith.

Introduction.

THAT the dielectric of a condenser becomes warmed when an alternating electromotive force is applied to the terminals of the condenser has long been known, and the study of this heating effect has been undertaken by numerous observers. Kleiner[1] used a thermo-electric couple imbedded in the dielectric to measure the rise of temperature, and noted a considerable heating effect in ebonite, gutta percha, glass and mica, but none at all in paraffin and rosin. He reports that in spite of all attempts by variations of the conditions of the experiment, no heating could be detected in these two last named substances. On the other hand, Boucherot[2] has made paraffin paper condensers for use on the 3200 volt commercial circuits of Paris, some of which became so hot in use that they were obliged to be cut out. Boucherot says of the heating effect that if a condenser rises as much as 30° C. it should be rejected. That this is good advice is evident from the fact that paraffin melts at 54° C., and hence when 30° C. above the temperature of a summer's day (say 25° C. or 77° F.) the paper would be floating in melted paraffin.

Bedell, Ballantyne and Williamson[3] report experiments upon a

[1] Wied. Ann., 50, p. 138.
[2] L'Eclairage Electrique, Feb. 12, 1898.
[3] Physical Review, October, 1893, vol. 1, p. 81.

paraffin paper condenser of 1.5 microfarad capacity the efficiency of which was found to be 95.6%, or 4.4% lost in heat. The loss was determined by a three-volt meter method, similarly to measurements on a transformer. It was put upon a 500 volt circuit at a frequency of 160 and the current was, therefore, about 0.7 ampère, the apparent watts about 350 and the heating effect 15.4 watts. The temperature rose one degree per hour.

Threlfall[1] reports a test on a paraffin paper condenser of his own construction which had a capacity of 0.123 microfarad, and on a circuit of 3000 volts and a frequency of 60 the rise of temperature was less than one-fifth of a degree per hour. The apparent watts would be about the same as in the experiment of Bedell, Ballantyne and Williamson, although the capacity was only one-twelfth as much. Threlfall concludes, apparently, that since his condenser had only one-twelfth the electric capacity, it also had only one-twelfth the capacity for heat of the other condenser; and since the temperature rose five times more slowly, therefore that the percentage loss of energy was only one-sixtieth as great, that is, $\frac{1}{60}$ of 4.4% or 0.07%; giving his condenser an efficiency of 99.93%. The reasoning is, however, quite inconclusive, for nothing is said concerning the thickness of the paraffin paper dielectric and the volume of the condenser. Suppose the dielectric of the smaller condenser to be 0.0129 inch thick, which is three times the thickness of the other. It might have been as thick as that, seeing it sustained for a considerable period an alternating voltage of 3000. Then the volume per unit of capacity would be nine times as great as the other, and the heat required to raise its temperature as rapidly would be nine times as great, assuming the capacity for heat and rate of radiation the same for both. The loss would then have been 0.63% instead of 0.07%. This illustrates how entirely inconclusive any determination merely of rise of temperature is, unless all such circumstances as heat capacity, rate of cooling, thickness of dielectric (or intensity of the electrostatic induction) are specified.

The enormous discrepancy between the results above referred to as to the quantity of the heating effect in condensers and the almost entire lack of precise statements as to its numerical value, led us to

[1] Physical Review, 1897, vol. 4, p. 458.

undertake more than a year ago, to measure this energy loss in such a way that it could be expressed absolutely.

We proposed to measure by means of a wattmeter the energy dissipated in a condenser when it is subjected to an alternating electromotive force. In order that the frequency of charge and discharge be perfectly definite, the electromotive force should be a simple harmonic one, that is, the upper harmonics of the fundamental should be absent. This is most easily effected by inserting in the circuit in series with the condenser a coil of wire having large self-induction but without an iron core. The variable permeability of the iron will give rise to upper harmonics, especially if the magnetic induction of the core attains large values, and hence a coil without an iron core is necessary. Moreover, if the self-induction is not large enough it will reinforce some of the upper harmonics, instead of quenching of them, and the presence of the coil will be detrimental rather than beneficial. The best value of the self-induction of the coil is such that the fundamental is reinforced to a maximum degree, in other words, it is that value of L given by the equation

$$t = 2\pi\sqrt{LC}$$

where t is the period of the fundamental component of the impressed electromotive force and C the capacity of the condenser.

There is another practical advantage resulting from this arrangement: namely, that the resulting resonance raises the electromotive force at the terminals of the condenser very greatly, saving the necessity of raising the voltage by transformers. And a third advantage now appears in the fact that the wattmeter may be inserted across the low voltage supply wires to measure the total power expended upon coil and condenser. Then subtracting the I^2r loss of the coil, the remainder will be the power expended upon the condenser. This supposes of course that there is no iron core and no eddy current loss in the copper coil itself.

The Resonance Method.

Fig. 1 shows the connections for this method, with the coil in series with the condenser. MN are the low voltage supply wires of an alternating circuit, S is an adjustable resistance, E is a dynamometer, F a wattmeter, R a noninductive resistance in shunt with the

coil and condenser, joining the points A and D. The wattmeter, therefore, measures the power expended between the points A and D, including the I^2r_w loss in the fixed coil of the wattmeter, but not including the i^2R loss in the shunt resistance R. The energy expended in the resonance coil and the fixed coil of the wattmeter, $I^2(r_c + r_w)$, subtracted from the total power measured leaves the condenser loss. This remainder is found to be also proportional to the square of the current. Hence we may write it I^2r_s, and r_s is the "equivalent resistance" of the condenser, which it is desired to find. This does not indicate the nature of the process by which energy is dissipated in a condenser, but simply that for a given condenser made of a given dielectric, at a given temperature and frequency, the heating effect is the same as though there were a certain resistance r_s in series with a perfect condenser of the same capacity. For the same dielectric r_s changes with changes in the temperature or the frequency; and for another dielectric with the same capacity, temperature and frequency, r_s would be different.

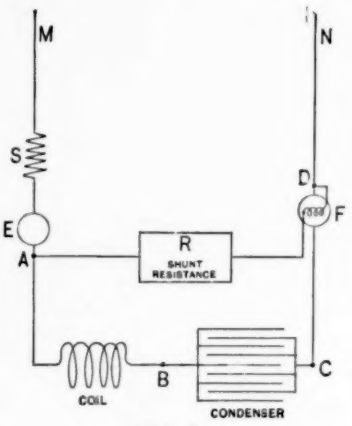

Fig. 1.

If $t = 2\pi\sqrt{LC}$, there is a complete resonance: $t = \frac{1}{n}$, and $p = 2\pi n$.

Therefore $\frac{1}{p} = \sqrt{LC}$, or $C = \frac{1}{p^2L}$. That is, for complete resonance the capacity is inversely proportional to the square of the frequency for a given self induction. If the frequency is fixed, either the capacity or the inductance may be varied until the current is a maximum; but if the frequency can be varied, the maximum resonance may be attained without varying C or L.

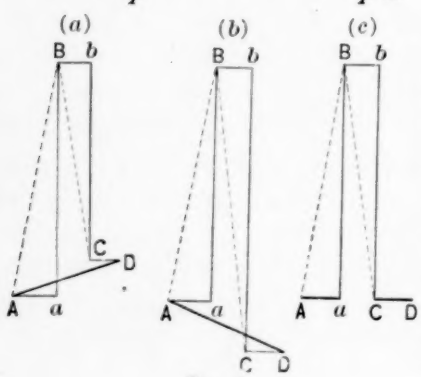

Fig. 2.

In Fig. 2 let $Aa = r_c$, the ohmic resistance of the coil.

$aB = pL$, the reactance of the coil.

$Bb = r_s$, the equivalent resistance of the condenser.

$bC = \frac{1}{pC}$, the condensance of the condenser.

$CD = r_w$, the resistance of the fixed coil of the wattmeter, its induction being negligible.

$AD =$ the resultant impedance of the circuit.

Then, if $pL = \frac{1}{Cp}$, the reactance is equal and opposite to the condensance (Fig. 2 *c*), the resonance is complete, and the impressed electromotive force, e, is expended in overcoming the resultant resistance $AD = r_c + r_s + r_w$, and $I = \frac{e}{r_c + r_s + r_w}$.

In Fig. 3, similar to Fig. 2 *c*, the several lines represent electromotive forces. $Aa = Ir_c = e_c =$ that part of the electromotive force expended in overcoming the ohmic resistance of the coil: similarly $Bb = e_s$, $CD = e_w$. Ba is the electromotive force (due to resonance) which overcomes the reactance, and bC is the electromotive force which overcomes the condensance. The potential of the point B varies through a wide range, whereas the points A, C and D, and the instruments suffer only small changes of potential.

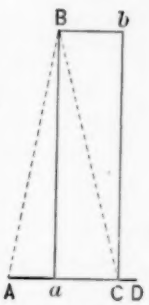

Fig. 3.

Advantages of the Method.

Herein lies one of the chief advantages of the method, that voltages below a hundred have to be dealt with at the instruments, whereas upon the condenser there may be an active electromotive force of several thousand volts. The noninductive resistance R is at most a few hundred ohms. On the contrary, if the shunt resistance were applied directly at the terminals of the condenser it would necessarily be several thousands of ohms, it must be capable of carrying the entire shunt current of the wattmeter, it must be strictly noninductive, and be of known value, conditions difficult to fulfil. In the resonance method a small inductance in the shunt resistance or the movable coil of the wattmeter produces no appreci-

able error, whereas in the simple wattmeter method it produces a large error.

To illustrate this point let us take a special case. Suppose the resistance R is 500 ohms, and the inductance of the shunt circuit, including both the resistance R and the movable coil of the wattmeter, is 0.003 henry, $2\pi n$ being 800. Then $pL = 2.4$ and $\tan \varphi_1 = 0.0048$, $\varphi_1 = 16' 30''$, the angle of lag of the shunt current behind the electromotive force. Suppose the true angle of the condenser current, φ_2, Fig. 4, is 89° 40′ ahead of the electromotive force. Then the difference of phase of the two currents in the wattmeter will $\varphi = \varphi_1 + \varphi_2 = 89° 56' 30''$ and the power factor, cos φ, of the expression, Watts $= EI \cos \varphi$, will be cos 89° 56′ 30″, instead of cos 89° 40′, that is .00102 instead of .00582; thus the wattmeter would indicate that the power absorbed in the condenser was only about one-sixth of what it really is. If the lag of the shunt current were more than 20′ (a not improbable value in many cases), the deflection of the wattmeter would be *negative!* It is possible that this explains why it has often been claimed that the loss in certain condensers is too small to be measured by a wattmeter. For example, Swinburne,[1] speaking of some of his own condensers, says "a condenser that takes 2000 volts and 10 ampères has a loss that is too low to measure—that is to say, it is less than 5 or 10 watts."

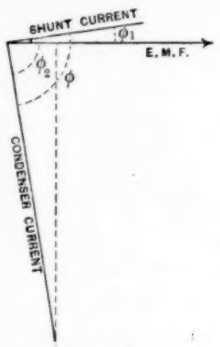

Fig. 4.

On the other hand, if by means of a resonance coil the current and electromotive force have been brought very nearly, if not exactly, into phase, any small lag of the shunt current will make no appreciable error. Thus, the cosine of 10° is .9848, and of 10° 16′ 30″ is .9840, a difference of less than one part in a thousand.

In order to determine the precise values of r_e and r_w a Wheatstone bridge is joined to AD and the condenser short-circuited, so that the resonance coil and the fixed coil of the wattmeter and the lead wires form the fourth arm of the bridge. The resistance is then quickly measured just after the wattmeter has been read and the

[1] Electrician, Jan. 1, 1892.

alternating circuit broken, and changes due to temperature are included.

Difficulty of the Method.

We have seen that the presence of a resonance coil in series with the condenser (1) quenches, to a large extent at least, the upper harmonics, (2) raises the voltage upon the condenser, thus avoiding transforming up, (3) enables measurements to be made more safely and more conveniently upon a low voltage, and (4) transfers the wattmeter problem from the most unfavorable case (where the angle of phase difference is nearly 90°) to the most favorable case where the current and electromotive force are nearly in phase. There is, however, one serious difficulty in the method. If the resonance coil is made of small wire it has a great resistance, and of the total power measured only a small part is expended upon the condenser. Thus the condenser loss is the difference between two relatively large quantities, and cannot be determined as accurately as could be desired. If, on the other hand, a large coil of larger wire is used so that its resistance is small, there will be eddy currents in the copper of the coil, and the power expended on the coil will be greater than I^2r_c. This excess will go into the remainder as condenser loss, and may give rise to a considerable error. If the wire is of large cross section, but stranded, so that its resistance is small, and the eddy currents negligible, then a large coil will have a large inductance, and no difficulty appears. The method is then accurate as well as quick and convenient.

The Resonance Ratio.

As the condenser is alternately charged and discharged energy is handed to and fro between the coil and the condenser. When the condenser is charged to its maximum extent the current is zero and all the energy is potential and residing in the condenser. A quarter of a period later the condenser is discharged and the current is a maximum; the energy is now kinetic and resides in the magnetic field of the resonance coil. At other instants the energy is partly potential (in the condenser) and partly kinetic (in the coil). As this transfer of energy to and fro continues, the dynamo supplying the current furnishes just enough energy to make good the losses, that

is, the heating effect in the wires and the dielectric of the condenser. The losses due to electromagnetic radiation and mechanical vibrations are usually negligible.

For the condenser alone, $I = \frac{E}{\sqrt{r_s^2 + \frac{1}{C^2p^2}}}$, I and E being the square root of the mean square values as indicated by an electrodynamometer and electrometer.

For the combined circuit,

$$I = \frac{e}{\sqrt{(r_w + r_c + r_s)^2 + \left(pL - \frac{1}{Cp}\right)^2}}$$

where e is the small impressed electromotive force and the denominator is the combined impedance of the circuit.

For complete resonance, $pL = \frac{1}{Cp}$, and hence $I = \frac{e}{r_w + r_c + r_s}$.

Hence,
$$\frac{E}{e} = \frac{\sqrt{r_s^2 + \frac{1}{C^2p^2}}}{r_c + r_s + r_w}$$

$$= \frac{\text{Impedance of the condenser}}{\text{Total resistance}} = \text{Resonance Ratio.}$$

In one case e was 50 volts and E was 2250, giving a resonance ratio of 45. The impedance was 51 ohms., $r_c + r_w$ was .38, r_s was .72. Hence, $\frac{51}{.38 + .72} = 46.4$, agreeing very nearly with the ratio of the voltages. In this case the coil was of large wire (No. 5, B. & S.) and had considerable eddy current loss. Hence, the value .72 for r_s was too large and the degree of resonance was lower than it would have been in the absence of eddy currents. In another case, using a coil of No. 10 wire, the impressed electromotive force was 29.5 volts, the voltage on the coil or the condenser was 1,808, and the resonance ratio $\frac{E}{e}$ was therefore 61.9.

The Resonance Coil in Parallel.

A second arrangement of the resonance coil is to put it in parallel with the condenser, Fig. 5, and impress upon both a high electromotive force. Each of the two parallel circuits, from B to C, takes its own current, independently of the other, but being nearly opposite in phase they nearly cancel each other in the supply wires. Hence, a small transformer is sufficient to supply the small current needed, although without the resonance coil a large transformer would be necessary. If, as

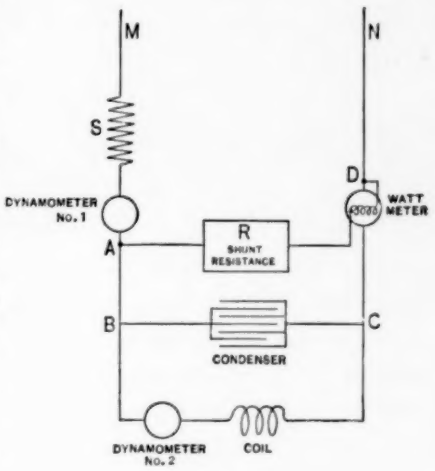

Fig. 5.

before, $pL = \frac{1}{Cp}$, there is complete resonance. The two parallel circuits, having the same impedance, take the same current; one current is nearly 90° ahead in phase of the electromotive force (see curve 2, Fig. 4), the other is nearly 90° behind (curve 3), the sum of the two being relatively very small (curve 4) and in phase with the impressed electromotive force (curve 1). The shunt resistance must be applied at the high voltage terminals, but as a small

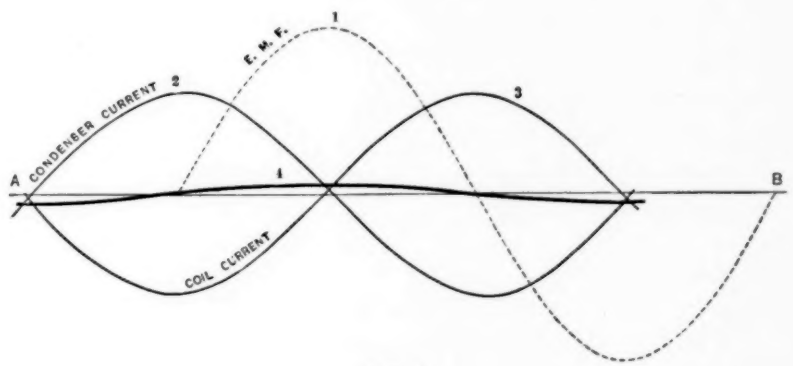

Fig. 6

amount of self-induction produces no appreciable error in the wattmeter, the movable coil may be long enough to make the wattmeter quite sensitive, and so a quite small shunt current is used. This requires a larger resistance, but with much smaller carrying capacity, since a much smaller shunt current will suffice.

To illustrate this point, suppose, as before, that for a given condenser the angle of advance of the current is 89° 40′. The power factor, cos φ, in the expression, Watts $= EI \cos \varphi$, is in this case .00582. If now a resonance coil be placed in parallel with the condenser and the current in the fixed coil of the wattmeter brought into phase with the electromotive force, then cos $\varphi = 1$. To get a certain deflection of the wattmeter, therefore, we must have the product of the two currents in the wattmeter nearly 200 times as great in the first case as in the second, and this requires a relatively large shunt current.

A modification of the method, if a second small transformer is available, is to transform down again to a low voltage, and put the shunt circuit of the wattmeter on the low voltage secondary of this second transformer. The current will now be almost exactly opposite in phase to the high electromotive force at the terminals of the condenser, and by interchanging the terminals the wattmeter deflection will be the same as before, if the shunt resistance is reduced in the ratio of transformation. The currents in the two coils of the wattmeter are so nearly in phase with one another that a small change in the phase of the shunt current will produce no appreciable error.

THE EFFICIENCY OF A CONDENSER.

Having thus determined the energy, w, dissipated in a condenser, by wattmeter measurements, we readily find r_s, the equivalent resistance of the condenser, from the expression $w = I^2 r_s$. The ratio of the equivalent resistance to $\frac{1}{Cp}$ is cot φ (fig. 7), φ being the angle of advance of the current ahead of the electromotive force. It remains to calculate the efficiency of a condenser.

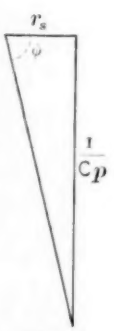

Fig. 7.

In Fig. 8 I is the current flowing into and out of the condenser, assuming both current and electromotive

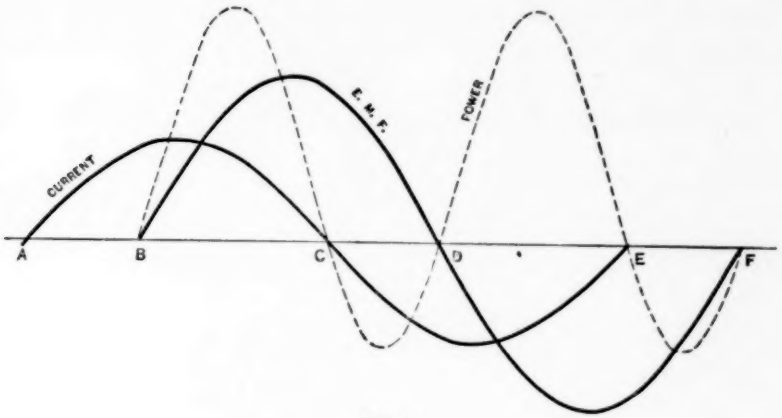

Fig. 8.

force to be simply harmonic. The dotted curve is the power curve.

$$e = E_1 \sin pt,$$

where $e =$ the instantaneous E. M. F. acting on the condenser, and E_1 is its maximum value.

$$i = \frac{E_1}{\text{Impedance}} \sin (pt + \varphi) = I_1 \sin (pt + \varphi)$$

$$ei = E_1 I_1 \sin pt \sin (pt + \varphi) = E_1 I_1 [\sin^2 pt \cos \varphi + \sin pt \cos pt \sin \varphi].$$

The area of the power curve for one half period, that is the area of the positive loop from B to C, minus the area of the negative loop from C to D is given by the integral

$$\int eidt = E_1 I_1 \cos \varphi \int_0^{\pi} \sin^2 ptdt + E_1 I_1 \sin \varphi \int_0^{\pi} \sin pt \cos ptdt$$

$$= \frac{E_1 I_1}{p} \cos \varphi \left[\frac{pt}{2} - \frac{\sin 2pt}{4} \right]_0^{\pi} - \frac{E_1 I_1}{4p} \sin \varphi [\cos 2pt]_0^{\pi} \quad (1)$$

$$= \tfrac{1}{2} \frac{E_1 I_1}{2n} \cos \varphi,$$

and this is the work done in $\frac{1}{2n}$ of a second.

Hence, for one second the work done, and, therefore, the power, is the well-known expression

$$\tfrac{1}{2} E_1 I_1 \cos \varphi = EI \cos \varphi, \qquad (E = E_1 \sqrt{\tfrac{1}{2}})$$

E and I being the effective values of the electromotive force and current.

The area of the positive loop, which represents the work done upon the condenser in charging it, is

$$\int_0^{\frac{\pi-\phi}{p}} eidt = \frac{E_1 I_1}{p}\cos\varphi\left[\frac{pt}{2} - \frac{\sin 2pt}{4}\right]_0^{\pi-\phi\,(\text{for } pt)}$$

$$-\frac{E_1 I_1}{4p}\sin\varphi\,[\cos 2pt]_0^{\pi-\phi\,(\text{for } pt)}$$

and the area of the negative loop, which represents the work done by the condenser in its discharge, is the value of the same integral between the limits $\pi - \varphi$, and π, for pt.

The efficiency is the ratio of the work done by the condenser in its discharge to the work done upon the condenser in charging it. This is the *gross efficiency*, η.

Taking $$\varphi = 89°, \eta = \frac{\text{Area of the Negative Loop}}{\text{Area of Positive Loop}}$$

$$= \frac{1/p\, E_1 I_1 \times .48638}{1/p\, E_1 I_1 \times .51378} = 94.67\%,$$

$$\varphi = 88°, \eta = \frac{.47287}{.52769} = 89.68\%,$$

$$\varphi = 87°, \eta = \frac{.45684}{.53905} = 84.75\%,$$

$$\varphi = 45°, \eta = \frac{.8584}{13.4248} = 6.38\%.$$

Having found the angle φ by the wattmeter measurements, the gross efficiency of the condenser may be found from the above equations or from Fig. 9.

SECOND DEFINITION OF EFFICIENCY.

Net Efficiency.

Regarding the condenser as an instrument for storing electrical energy, and in which a certain amount of energy is dissipated in the process, we may define the percentage of loss as the ratio of the energy dissipated to the energy stored. The efficiency is then unity

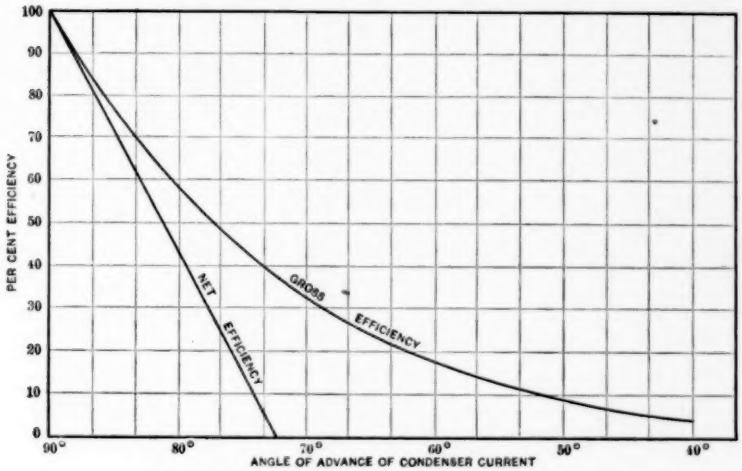

Fig. 9.

minus the loss, or in per cent., the efficiency is 100 minus the per cent. of energy dissipated. This we may call the net efficiency and represent it by ε. Then,

$$\varepsilon = \frac{\text{Energy Stored} - \text{Energy Dissipated}}{\text{Energy Stored}}, \text{ or,}$$

$$1 - \frac{\text{Energy Dissipated}}{\text{Energy Stored}}.$$

In figure 10 $I_1 r_s = E_1 \cos \varphi$ = active electromotive force. $\frac{I_1}{Cp} = E_1 \sin \varphi$ = wattless electromotive force, or the E. M. F. which charges the condenser.

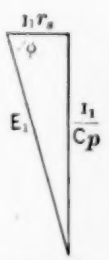

Fig. 10.

The maximum charge of the condenser is

$$CE_1 \sin \varphi.$$

and its energy, $W = \frac{1}{2} CE_1^2 \sin^2 \varphi.$

The expression for the power is

$$\tfrac{1}{2} I_1 E_1 \cos \varphi.$$

But $I_1 = CE_1 \sin \varphi \cdot p.$

Hence the energy dissipated per second is

$$\tfrac{1}{2} CE_1^2 \sin \varphi \cos \varphi \cdot p,$$

and the energy dissipated per half period (that is during the time of a single charge and discharge) is

$$w = \tfrac{1}{2} CE_1^2 \sin \varphi \cos \varphi \cdot \pi.$$

The relative loss is, therefore,

$$\frac{w}{W} = \pi \cot \varphi$$

and the net efficiency is

$$\varepsilon = 1 - \pi \cot \varphi.$$

For

$\varphi = 89°$	$\varepsilon = 94.52\%$
$\varphi = 88°$	$\varepsilon = 89.03\%$
$\varphi = 87°$	$\varepsilon = 83.54\%$
$\varphi = 72°20'30''$	$\varepsilon = 0$

The net efficiency, ε, is, therefore, slightly less than the gross efficiency, η, for values of φ nearly 90°, but as the angle φ diminishes, ε falls rapidly below η and for $\varphi = 72°20'30''$ the energy dissipated is equal to the energy stored, and the net efficiency is, therefore, zero, while the gross efficiency is about 38 %. For greater angles of lag the loss is greater than the maximum energy stored, and ε becomes negative, see Fig. 9.

For ordinary cases the angle φ is greater than 88° and ε and η are nearly equal. Since the wattmeter method gives directly the value of φ it is much easier to express the value of the net efficiency ε, $(1 - \pi \cot \varphi)$, than the value of η. For small values of cot φ this is sufficiently exact to write it $\varepsilon = 1 - \pi \cos \varphi$.

Suppose that instead of assuming the effective resistance of the condenser to be in series with the condenser, as we have done in figures 2, 3, 7 and 10, we consider that it is in parallel, as in figure 11. Of course there is a slight leakage current in every case, if the resistance of the dielectric is not infinite. Boucherot[1] says of his paraffin paper condensers that the "heating is chiefly due to the Joule effect, that is, to leakage current; the action of dielectric hysteresis, if it exists at all, being very slight." We shall give reasons in a subsequent paper for believing

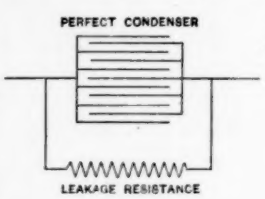

Fig. 11.

[1] L'Eclairage Electrique, Feb. 12, 1898.

that this is seldom, if ever, true of good condensers, but at present let us assume it to be true. Then the condenser current is 90° ahead of, and the leakage current in phase with, the impressed electromotive force. I being the total current, the condenser current is $I \sin \varphi$ and the leakage current is $I \cos \varphi$. The energy stored is

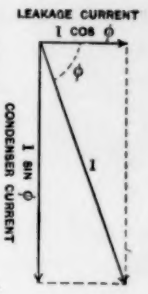

Fig. 12.

$$W = \tfrac{1}{2} C E_1^2$$

and the energy dissipated per second is

$$\tfrac{1}{2} E_1 I_1 \cos \varphi$$

or per half period,

$$w = \frac{1}{4n} E_1 I_1 \cos \varphi.$$

The maximum condenser current, $I_1 \sin \varphi$, $= pCE_1$.

$$\therefore w = \frac{E_1^2 pC \cos \varphi}{4n \cdot \sin \varphi} = \tfrac{1}{2} C E_1^2 \pi \cot \varphi$$

$$\because \varepsilon = 1 - \frac{w}{W} = 1 - \pi \cot \varphi, \text{ as before.}$$

Referring to Fig. 13, we can derive anew the value of the net efficiency. Curve 1 is the electromotive force. Curve 2 is the current, in advance in phase by the angle φ, nearly 90°. Curve 3 is the power, the positive and larger loop being the work done on the

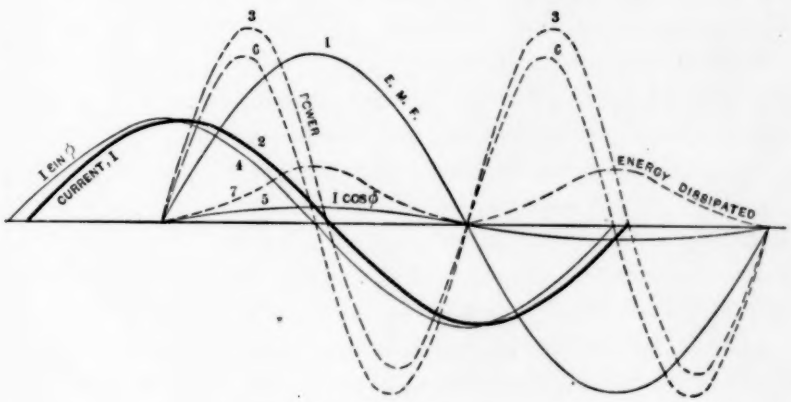

Fig. 13.

condenser and the negative and smaller loop being the work done by the condenser, upon its discharge. Equation (1) p. 11, shows the area of this power curve to consist of two terms, the coefficient of one containing $I \cos \varphi$, and of the other, $I \sin \varphi$. Now $I \cos \varphi$ is the component of the total current which is in phase with the E. M. F. and is represented by curve 5. $I \sin \varphi$ is the condenser current, 90° ahead of the E. M. F., and is represented by curve 4. The power curve for 4 is 6; its positive and negative loops are equal, and it is the power curve for a perfect condenser. The power curve for 5 is 7, and is the total work done, or the energy dissipated. One loop of 6 is energy stored, W, one loop of 7 is energy dissipated, w, and the ratio $\frac{w}{W}$ is the relative loss, or $1 - \frac{w}{W}$ is the net efficiency.

The area of one loop of 7 is

$$w = \frac{E_1 I_1 \cos \varphi}{p} \left[\frac{pt}{2} - \frac{\sin 2pt}{4} \right]_0^{\pi}$$

$$= \frac{E_1 I_1 \cos \varphi}{p} \frac{\pi}{2} = \text{Energy Dissipated.}$$

The area of one loop of 6 is

$$W = \frac{E_1 I_1 \sin \varphi}{4p} \left[\cos 2pt \right]_0^{\frac{\pi}{2} (\text{for } pt)} = \frac{E_1 I_1 \sin \varphi}{4p} \cdot 2 = \text{Energy Stored.}$$

$$\therefore \varepsilon = 1 - \frac{w}{W} = 1 - \pi \cot \varphi,$$

as before.

If the equivalent resistance of the condenser is taken to be a series resistance, then we have $E \cos \varphi$ for the active E. M. F., $E \sin \varphi$ for the condenser E. M. F., and the same result follows.

EXAMPLE OF THE RESONANCE METHOD.

The condenser used was one which we had made ourselves, and consisted of paraffined paper and tinfoil. The paper was 12 by 17 cm. and .0038 cm. thick: the tinfoil was .0025 cm. thick, and its effective area was 10 by 15 cm. approximately. The paper and tinfoil were piled up dry and clamped between brass plates. It was

then placed in melted paraffin and maintained for some hours at 100° to 150° C. This condenser then had a volume of about 300 cc. and a capacity of about .8 microfarad.

The resonance coil consisted of 3000 meters of No. 10 wire (B. and S. gauge, .259 cm. diameter) wound into a coil of 40 cm. internal diameter, 56 cm. external diameter and 17 cm. axial length. Its resistance was about 10 ohms and its inductance 1.60 henrys. This coil was wound in ten sections, so that by choosing different sections or combination of sections, a wide range of inductance could be secured. In this particular case the entire coil was used. The frequency of the alternating electromotive force was varied by varying the speed of the generator, complete resonance being attained at a speed of 2175, for which the frequency is 145. The current was 1.20 ampères, the resistance, $r_c + r_w$, was 9.82 ohms, $I^2(r_c + r_w) =$ 14.15 watts. The wattmeter gave a deflection of 188, corresponding to 37.6 watts. This leaves 23.45 watts for condenser loss, or $EI \cos \varphi$.

$$E \text{ being } 1808,\ I = 1.20,\ EI = 2169 \text{ and } \cos \varphi = \frac{23.45}{2169} = .0108$$

$$\pi \cot \varphi = 3.39\,\%,\ \varepsilon = 96.61\,\%.$$

The quantity of the eddy current loss in the coil does not of course appear. From subsequent experiments we became satisfied that it was large enough to cause a serious error in the above value of the condenser loss. Hence we shall not give any of the other values found using this coil. The results obtained over a range of from 400 to 2250 volts showed that the loss is sensibly proportional to the square of the electromotive force. This conclusion is not seriously affected by the presence of eddy currents, since the latter are themselves proportional to the square of the E. M. F. and yet are not large enough to swamp the condenser loss.

We therefore wound up a coil of nearly 2000 meters No. 14 wire (B. & S. gauge, diam. .160 cm.), in 41 layers of 45 turns each, external diameter of the coil being 37 cm. The eddy current loss in this coil is less, owing to the smaller diameter of the wire, and the smaller quantity; a subsequent measurement by an independent method gave 3.2 % as the increase of the effective resistance by the

eddy currents at a frequency of 120. At a lower frequency it would, of course, be less. Its use will therefore illustrate the method and give a fairly accurate value of the condenser efficiency.

MEASUREMENTS ON BEESWAX AND ROSIN CONDENSERS.

We give below a series of measurements on the efficiency of a set of commercial condensers made of tinfoil and paper, the latter being saturated with melted beeswax and rosin. We understand that they are piled up dry, as we have done with condensers made in our own laboratory, and while immersed in the melted beeswax and rosin placed in a receiver from which the air is exhausted to free them from air and moisture. With the details of the process we are not, however, acquainted, and cannot say whether it is something in the method of manufacture or the nature of the dielectric which makes the dissipation of energy so large; we presume, however, that it is the latter. There were six condensers joined together, each being a solid slab of about 11 cm. by 15 cm. by 1.5 cm., thus having a volume of about 250 cc., and a capacity of one-third of a microfarad.

The six slabs were placed on a table, joined together in parallel, and in series with the resonance coil (which was at a distance from them and from the measuring instruments), loosely covered with a woolen cloth, and coil and condenser subjected to an alternating electromotive force of about 50 volts, and a frequency of about 120. No effort was made to secure the maximum degree of resonance, and the voltage on the condensers was found to be about 900. In a short time the temperature of the condensers had risen to 30°, as indicated by the thermometer inserted between two of them, and the first set of readings was taken. The loss of energy in the condensers was greater than it had been at lower temperatures, and continued to increase as the temperature rose.

At the same time, owing to this increase in the equivalent resistance of the condenser, the resonance ratio decreased and the current and voltage on the condenser decreased. The loss at 36° C. is 50 % greater than at 30° C., and is approaching a maximum. At 39° it is 9.5 % and the fourth reading at sensibly the same temperature (but which doubtless was a little higher, at least in some of the con-

densers) showed a slightly less loss. At 47° C. the loss had decreased to 8 %, and at 49°.5 C. to 6.5 %, only two-thirds its maximum value. The condensers were not all at the same temperature, and the indicated temperatures are, therefore, not exact. But they show unmistakably a maximum value of the condenser loss, or energy converted into heat, at about 39° C. and beyond that a very considerable diminution. No further readings were taken until the condensers had risen several degrees, when it was suddenly noticed that one pair was hotter that the others and getting soft.

TABLE OF MEASUREMENTS ON BEESWAX AND ROSIN CONDENSERS.

a	b Temperature of the Condenser.	c Current through the coil and condenser. I	d Resistance of coil and Wattmeter. $=r_c+r_w=$	e $I^2(r_c+r_w)=W_1$	f Wattmeter Readings.	g Total Watts. W	h Watts in condenser. $W_2=W-W_1$	i Voltage on condenser. E	j Apparent Watts. $E\times I$	k $\cos\phi=\frac{W_2}{EI}$	l Per cent. Loss $=100\pi\cot\phi$	m Net Efficiency $100(1-\pi\cot\phi)$.
1	30°	1.53	17.18	40.2	129½	66.70	26.50	910	1392	.0190	5.97	94.03
2	36	1.16	17.92	24.1	90	46.35	22.25	669.5	776.5	.0287	9.05	90.95
3	39	1.34	17.96	32.22	122	62.83	30.61	752	1007	.0304	9.55	90.45
4	39	1.27	17.96	28.97	108	55.62	26.65	712	904	.0295	9.27	90.73
5	47	1.31	18.22	31.26	108	55.62	24.36	731.4	958	.0253	7.95	92.05
6	49½	1.50	18.42	41.44	131	67.46	26.02	839	1259	.0207	6.49	93.51
7	59	0.80	18.34	11.74	173	89.10	77.36	433	346	.2230	71.85	28.15

The thermometer in a cooler pair registered 59°, but the warmest pair was considerably higher. The loss was astonishingly large, but the condenser had not broken down. Moreover, the "leakage current" had not greatly increased, for while 839 volts gave 1.50 ampères at 49°.5, 433 volts gave .80 ampère at 59° C. To be in exact proportion to the voltage the current should have been .774 instead of .80 at the higher temperature, a comparatively small difference.

To find so large a loss in commercial condensers of good repute was a surprise to us. To find a well-marked maximum as the temperature rose, beyond which the loss decreased as the beeswax and rosin composition softened was a second surprise. To find so large a loss as the last observation shows without the condensers giving way, and without any very large leakage current, was a third surprise.

In order, therefore, to verify these results by a totally different method, and to determine as accurately as possible the losses in some paraffin paper condensers which we possessed which showed relatively very small heating effects, we built a special form of calorimeter, into which the condensers could be placed and the heat directly measured. The calorimeter was copied after the large respiration calorimeter which one of us designed for experiments under the patronage of the United States Government, and which is located at Wesleyan University. The description of the calorimeter and the results obtained with it are reserved for a subsequent communication. We will only add that they fully confirm the unexpected results obtained by the Resonance Method given above concerning the dissipation of energy in beeswax and rosin condensers.

WESLEYAN UNIVERSITY, MIDDLETOWN, CONN., July 1, 1898.

ON THE DENSITY OF ICE.

By Edward L. Nichols.

I. A Review of the Existing Data.

MY attention was first drawn critically to the question of the density of ice by the necessity of using this value in computing the results obtained with the ice calorimeter. Since the introduction of Bunsen's instrument the value which he determined, namely 0.91674, seems to have been universally accepted as the proper quantity to use in such computation. At first sight the accuracy of this value would appear to leave nothing to be desired, for, although it is based upon three determinations only, these are in agreement with themselves to about one part in ten thousand. Bunsen himself calls attention, however, to the widely different values obtained by earlier experimenters; values which range between that obtained by Heinrich (0.905) and that given by Dumas (0.950). It is to be remembered, moreover, that Bunsen's three values were obtained by repetitions of the same procedure without any variation whatever. Such repetitions might well give accordant values even where the method was subject to a considerable systematic error.

A careful perusal of the work of some of the other investigators in this field would lead us to feel an almost equally great confidence in the result which they have obtained. The determination of Brunner, for example, which led to the value 0.9180, is based upon a much more extended series of observations than that of Bunsen, and the agreement of the various series among themselves is quite as satisfactory.

So long as the ice calorimeter is to be used simply for comparing the thermal capacities of various substances with that of water, it is possible to calibrate it by the use of known quantities of water at known temperatures, and thus to avoid the necessity of an acquaintance with the absolute value of the density of ice. If we propose

however to use the ice calorimeter for absolute determinations in heat the absolute value of the density of ice becomes an important factor. It seemed desirable therefore to look carefully into the literature of this subject, upon which so many investigations have been made, in the hope of determining whether the widely divergent results obtained owe their difference to systematic errors in the methods employed, or whether, as would appear from the results reported by some observers, different specimens of ice really possess widely different densities.

The first attempt to measure the density of ice appears to have been made by Robert Boyle, whose experiments are described in his "Experimental History of Cold."[1] Boyle found that by freezing water in glass vessels by the application of a mixture of snow and salt applied from below, it was possible to entirely congeal the liquid without breaking the vessel; and that under such circumstances the ice rose higher in the neck of the vessel than did the unfrozen water. To determine the increase, he calibrated a glass vessel with a long neck, and nearly filled the same with water freed from air by means of his "pneumatical engine" (air pump). As congelation progressed, he noted the height of the ice in the neck. He found that eighty-two parts of water made 91 1/8 parts ice; or in other words that the density of ice was almost exactly 0.903. Kraft,[2] another of the earlier observers, obtained a nearly similar value [0.905]. Dr. Irving, who accompanied Phipps on his voyage to the Arctic Ocean (1773), determined the density of the Arctic ice. His experiment is thus tersely described in Phipp's Journal: "A piece of the most dense ice he could find being immersed in snow water (thermometer 34°) fourteen-fifteenths sunk under the surface; in brandy it barely floated; in rectified spirits of wine it fell to the bottom at once and dissolved immediately."[3] The value of the specific gravity found by Irving [0.937] was presumably that of the Arctic ice. Scoresby, according to the article in Gehler's Wörterbuch,[4] just cited, found nearly the same value.

[1] Boyle's Works (Edition of 1772); Vol. II., p. 551.
[2] Gehler; Physikalisches Wörterbuch, Vol. III., p. 113.
[3] Pinkerton's "Voyages and Travels;" Vol. I., p. 573.
[4] Gehler, Physikalisches Wörterbuch (l. c.).

Of the various other early observers whose results have been recorded, Williams[1] found the density of ice to be 0.945; Thomson[2] 0.92; Heinrich,[3] of whom mention has already been made, 0.905. Berzelius,[4] in his treatise on chemistry, gives as the specific gravity of ice, 0.916, but he does not state from whose determination this value was obtained. Dumas,[5] on the other hand, gives 0.95 as the proper value; likewise without any reference to the original work.

If we select from the work done upon the density of ice during the present century those investigations which make pretension to scientific accuracy, we find that they may be classified, according to the method pursued into three groups:

Thomas Thomson in the second volume of his treatise on chemistry,[6] describes experiments in which he made mixtures of water and alcohol, varying the density until ice submerged in the liquid was in equilibrium; he then determined the specific gravity of the mixture, which he found to be 0.92. Dufour in 1860–62 made redeterminations by this method. In his first experiments he found it necessary, on account of the rapid solution of the ice by the alcohol, to work at temperatures several degrees below the melting point. It was thus that the results which he reported in 1860 were obtained.[7] He found the density of the liquid within which ice was in equilibrium always to lie between 0.922 and 0.914; the mean of twenty-two experiments gave 0.9175 ± 0.0007. In 1862 Dufour[8] reported upon a continuation of his work in which chloroform and petroleum, neither of which dissolves ice, were used. The density of the mixture was found by weighing glass, the specific gravity and coefficient of expansion of which had been previously determined, in the same. The temperatures at which determinations were made varied from $-5°$ to $-8°$ and the results were reduced

[1] Williams, Gothaisches Mag., Vol. VIII., p. 176.

[2] Th. Thomson, A System of Chemistry (Edinburgh, 1802), Vol. II., p. 167.

[3] Heinrich, Bayerisches Akad., 1806, II., p. 149.

[4] Berzelius, Lehrbuch der Chemie (Wöhler's translation, 1825), Vol. I., p. 364.

[5] Dumas, Traité de Chimie, Vol. I., p. 19.

[6] Thomson, l. c.

[7] Dufour; Comptes Rendus, Vol. L., p. 1040. [See also Bibliotheque Universelle; Archives des Sciences, Vol. VIII. (1860), p. 89.]

[8] Dufour; Comptes Rendus, Vol. LIV., p. 1080. [See also Archives des Sciences, Vol. XIV. (1862), p. 5.]

to zero by using Plücker's value of the coefficient, namely 0.000158. He found as upper and lower limits the density of the liquid in which ice was in equilibrium 0.9207 and 0.9133; the mean value obtained from sixteen experiments was 0.9178±0.0005.

The second of the three modern methods of procedure was that followed by Brunner.[1] It consisted in weighing in air and in a liquid incapable of dissolving ice, the specimens the densities of which were to be determined. His experiments were directed primarily to the determination of the coefficient of expansion and particularly to the refutation of a statement put forth in the preceding year by Petzholdt to the effect that ice expands on cooling. His measurements led, however, directly to the specific gravity of ice at various temperatures from $-1°$ C. to $-20°$ C. From these, having determined the coefficient, he was able to compute, with what appears to be a high degree of precision, the density of the ice at zero.

The specimens of ice subjected to experiment were suspended from the balance arm by means of a human hair. The liquids in which they were weighed were refined petroleum and in some instances oil of turpentine. The density of these liquids at various temperatures was measured by the method of weighing, submerged in them, a piece of glass, the coefficient of which had been carefully determined. Measurements of the density of the oil were always made immediately before and after the weighing of the submerged ice. It was found that at temperatures above $-1.5°$ turpentine oil begins to attack the ice. In the case of petroleum no such phenomenon was noted. The specimens which had been submerged in petroleum for 24 hours showed no loss. All weighings were reduced to weight in vacuo. The linear coefficient of expansion of the ice, computed by Brunner from seven series of weighings, was found to be 0.0000375.

Brunner, like nearly all the other experimenters, who have attempted to produce artificial ice for the purpose of such determinations, found that despite all precautions ice thus made from recently boiled distilled water was full of minute cracks which in his estimation rendered it useless. He therefore rejected artificial ice in favor

[1] Brunner, Poggendorff's Annalen; Vol. CXL., p. 113, 1845.

of selected specimens of river ice, produced by rapid freezing during the first days of a period of extremely cold weather. He found it possible to obtain pieces which were of sufficient size for use by his method and which were entirely clear and free from bubbles.

The third method to be considered consisted in the use of the dilatometer. This instrument in one form or another has been employed by Plücker and Geissler (1852), Kopp (1855) and Bunsen (1870). The instrument employed by Plücker and Geissler[1] was a thermometer-like dilatometer, the bulb of which was entirely filled with distilled water and mercury. The latter liquid extended to a certain point in the long capillary neck of the instrument. This dilatometer doubtless owed its highly ingenious and interesting construction to Geissler, whose skillful workmanship in glass is well known. The construction is shown in Fig. 1. The bulb *B* was completely filled with mercury after which water was introduced through the capillary tube *C*. The latter liquid entered only an inner reservoir formed by surrounding the capillary tube with a larger tube of thin glass, which was open at the lower end. As soon as the mercury had been displaced from this inner chamber, the tube *C* was sealed off and the instrument was placed in an ice bath. The height of the mercury in the capillary neck at 0°C. was then noted. The dilatometer was subsequently transferred to a bath of alcohol which had been cooled by the use of ice and calcium chloride. The result of freezing was to fracture the walls of the inner chamber so that the instrument could be used only for a single determination. From the movement of the mercury in the capillary neck the change of volume of the ice was determined. Three experiments gave for the volume of ice, in terms of that of the water frozen; 1.09192, 1.09197 and 1.09195, from which the specific gravity in terms of that of water at 0° is 0.91582, 0.91578 and 0.91579; the mean of the three determinations is nearly 0.91580.

Fig. 1.

Plücker and Geissler likewise determined the cubic coefficient of expansion of the ice, which they found to be equal to 0.000158.

[1] Plücker and Geissler: Poggendorff's Annalen, Vol. LXXXVI., p. 265.

The corresponding linear coefficient [0.000052] is considerably larger than that obtained by Brunner by the method of weighings.

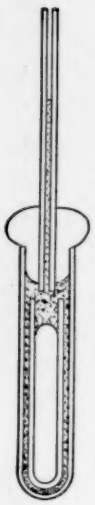
Fig. 2.

The second experimenter by the method of the dilatometer, Kopp,[1] used an instrument in no wise comparable in delicacy with that of Plücker and Geissler. Kopp's dilatometer (Fig. 2) consisted simply of a test-tube-shaped tube of glass which was closed by a carefully fitted and especially prepared cork, through which cork the capillary neck was inserted. An inner tube of the same general shape contained the liquid, the change of volume upon the freezing of which was to be determined. The ice employed was obtained from distilled water, rendered air-free by boiling, which was frozen during the experiment. It was usually clear excepting for the presence of one minute bubble. The liquid with which the dilatometer was filled was turpentine oil; the experiments were made at temperatures varying from $-3°$C. to $-5°$C. The inner cylinder was intended, as in the case of Plücker and Geissler's instrument to prevent the fracture of the outer glass upon freezing. Kopp found, however, that the inner tube did not break. His values, from two sets of readings, uncorrected for temperature, were respectively:

$$\frac{\text{Vol. of water at } 0°}{\text{Vol. of ice at } -3°} = \frac{1}{1.100} = 0.909$$

Mean = 0.908.

$$\frac{\text{Vol. of water at } 0°}{\text{Vol. of ice at } -3°} = \frac{1}{1.103} = 0.907.$$

If we reduce this value to 0° by the application of the coefficient of expansion given by Plücker and Geissler we find for the specific gravity of ice, 0.9078.

The dilatometer used by Bunsen in his well-known experiments, is described in the opening paragraphs of his paper on the Ice Calorimeter.[2] It consists of a long U-shaped tube with thick walls, one end of which was drawn out into a stout capillary point and

[1] Kopp, Annalen der Chemie und Pharmacie, Vol. XCIII., p. 129 (1855).

[2] Bunsen; Calorimetrische Untersuchungen; Poggendorff's Annalen, Vol. CXLI., p. 1 (1870).

bent over into a nearly horizontal position, as shown in Fig. 3. The apparatus was filled to the level BB' with mercury which was boiled to free it from air. The arm AB was subsequently entirely filled with distilled water and the opening at A was closed by the application of the Bunsen flame. The other arm of the tube was then entirely filled with boiled mercury and was closed by the insertion of a carefully fitted cork through which passed a capillary tube F. This tube and the small mercury reservoir C, into which it dipped, are shown in the drawing. In order to freeze the water in the right arm of the U-tube without fracturing the tube, the apparatus was nearly buried in sawdust, the upper end only being exposed to a temperature below 0°. After the water within the exposed portion was solidified the tube was raised gradually, whereby the ice column was made to extend downwards until it reached the level of the mercury and the entire mass of water was frozen. By weighing the apparatus before and after the introduction of the liquid the mass, and by computation, the volume of the water was determined. By weighing the mercury receiver C before and after the operation of freezing, the increase in volume due to the conversion of the water into the solid state was measured. Bunsen made three determinations with this apparatus; as the result of which he found the specific gravity of ice to be: 0.91682, 0.91673, 0.91667. The mean of the three determinations was 0.91674. The density is expressed in terms of that of water at 4° C.

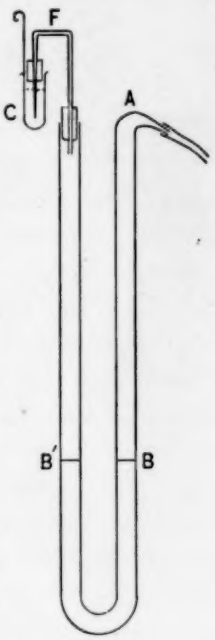

Fig. 3.

To facilitate the comparison of the results obtained by the three methods just described the various data are brought together in the table on next page.

From the inspection of this table it appears that the determinations of Brunner, of Plücker and Geissler and of Bunsen should be given greater weight than those of Kopp and of Dufour.[1] The

[1] Thomson's result, which is given only to the second place of decimals, I have omitted altogether, as of uncertain value.

probable errors have been computed from the available data, excepting in the case of Dufour's results, where the errors given by the author have been used.

TABLE I.

A compilation of the results of various modern determinations of the density of ice.

Date.	Observer.	Method.	Kind of Ice.	Sp. Gr. at 0°	Mean.	Probable Error.
1845	Brunner.	Weighing in petroleum or turpentine oil.	River ice free from bubbles.	0.9180	0.9180	±.000039
1852	Plücker and Geissler.	Dilatometric	Ice from boiled water frozen in the bulb.	0.91582 0.91578 0.91580	0.91580	±.000008
1855	Kopp.	Dilatometric	Ice from boiled water frozen in the bulb.	0.909 0.907	0.9078	±.0007
1860 to 1862	Dufour.	Floating in alcohol and water.	Ice from boiled water	22. Exps.	0.9175	±.0007
		Floating in chloroform and petroleum.		16. Exps.	0.9178	±.0005
1870	Bunsen.	Dilatometric	Ice from boiled water frozen in the instrument.	0.91693 0.91684 0.91678	0.91685	±.00003

The various results differ from each other by amounts so greatly in excess of the probable errors of each set as to indicate the existence of considerable causes of divergence other than the ordinary errors of observation. In how far we have to do, in the consideration of these results, with systematic errors, inherent in the various methods employed, and in how far with differences in the density of the specimens of ice, it is hard to say. It is noteworthy that there are three kinds of ice, in question, viz., natural ice (Brunner), artificial ice previously prepared (Dufour), and ice produced within the dilatometer during the progress of the experiment (Plücker and

Geissler, Kopp, Bunsen). The first of these appears to have the greatest, the last named the smallest, density.

If we ignore these considerations altogether and assign weights based upon the size of the probable errors, the most probable value for the density of ice computed from all the data is

$$\text{Sp. Gr. ice} = 0.91593 \pm .0000079.$$

Too great confidence should not be placed in the accuracy of this value. The redetermination of the density of ice to be described in a subsequent portion of this paper shows unmistakably that natural ice is denser by more than one part in a thousand, than the artificial ice produced in the ice calorimeter. There is reason to think, moreover, that the exceedingly small probable error of Plücker and Geissler's value arises from the fortuitous agreement of their results. Such a coincidence is by no means extraordinary where only three determinations are recorded. A careful reading of the details of the two investigations has led me to the opinion that the observations of Plücker and Geissler do not appreciably exceed those of Bunsen in accuracy. If we exclude the results of Brunner and Dufour, and consider only data obtained with ice formed directly within the dilatometer, and if we assign weights arbitrarily, in accordance with the view just expressed (Plücker and Geissler 25, Bunsen 25, Kopp 1), we arrive at the following result :

$$\text{Sp. Gr. ice formed in dilatometer} = 0.91614 \pm .00002.$$

II. A Redetermination of the Density of Natural and of Artificial Ice.

In order to obtain a satisfactory answer to the questions which I had in mind in undertaking this investigation, namely whether the density of ice is the same under all circumstances, and if so, what the proper value for the density is, it seemed important to make a redetermination with a special view to the measurement of the ice actually produced in the ice calorimeter, and a comparison of its density with that of ice formed under other conditions.

The most obvious method of performing this operation consists in freezing the ice mantle around the inner tube of the calorimeter

in the usual way and then, having displaced the unfrozen water from the instrument as completely as possible and having frozen those portions which cannot be thus displaced, to fill the remaining space with mercury. The weight of this mercury together with that of the ice which it contains would give the data for the computation of the density of the ice mantle. I made a large number of measurements of this kind, with results which agreed excellently among themselves, but which were so widely at variance with the results obtained by Bunsen and the other experimenters quoted in the opening paragraph of this paper as to cast suspicion upon the method. Three experiments, for example, in which this method was employed and in which corrections for the errors of the weights used, together with reductions of all weighings to vacuo were made, gave for the density of the ice mantle in an ice calorimeter 0.92371, 0.92364 and 0.92386. These values agree with one another quite as closely as those given by Bunsen in his paper, but they differ from his values, by a quantity more than fifty times as great as the probable error of each taken by itself.

The ice mantles obtained for the purpose of these measurements were made by introducing into the inner tube of the calorimeter a mixture of ether and of carbon dioxide. This results in very rapid formation of ice and the mantle, when a certain size is reached, invariably becomes filled with a network of fine cracks. In order to ascertain whether the high density found in the measurements just quoted are due to the much lower temperature at which the ice was formed, an ice mantle was produced following the method described by Bunsen in his paper, namely, by the introduction of alcohol cooled to a temperature of a few degrees below zero. The rapidly formed ice in the experiments in which ether and carbon dioxide had been used was found to be denser by about one part in a thousand than that obtained by freezing with the cooled alcohol, a difference by no means sufficient to explain the discrepancy between my results and those of Bunsen. A comparison of these measurements with those subsequently obtained with other ice calorimeters showed that the error was one which depended upon some individual characteristic of the apparatus employed, and it was not difficult to trace it to deformation of the glass vessel under the

weight of the mercury which filled it. The size of these calorimeters was such that they would contain over 2,000 grams of mercury. It was found by varying the position of the calorimeter at the time of filling that considerable discrepancies in the contents occurred.

To settle more definitely the question of the influence of the deformation of the walls of the calorimeter, I had a set of specific gravity flasks made of the form shown in Fig. 4. These were constructed with a tube similar to the inner tube of an ice calorimeter inserted through the bottom. After being filled with boiled water and inverted, an ice mantle could be formed around the inner tube in the usual manner. The water expelled by the ice during the growth of the mantle escaped through the capillary opening in the stopper of the flask.

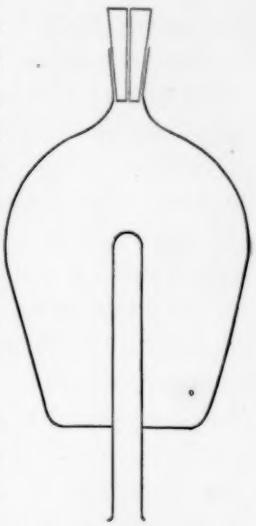

Fig. 4.

A flask of this kind was weighed empty. It was then calibrated by filling completely with mercury at 0° and weighing the mercury.

The flask was then filled with water, carefully boiled. It was then inverted and an ice mantle was formed by pouring ether and solid CO_2 into the inner tube. The stopper was then withdrawn and the unfrozen water was removed, so far as possible, by vigorous shaking of the flask. What remained was frozen by exposure to cold and the flask, together with the ice which it contained, was weighed in a room, the temperature of which was below 0°. Mercury, likewise considerably below 0° in temperature, was poured into the flask until the latter was completely filled with ice and mercury. It was then inverted in a beaker of mercury, with the stopper submerged, and was left in an ice bath in that position over night. The flask, with its contents, was then reweighed in the cold room, and inverted with stopper submerged in a weighed beaker of mercury, in which position the ice was allowed to melt and the flask was again brought to 0°. The loss of weight of the beaker of mercury gave a measure of the change of volume due to the liquefaction of ice. Finally, the whole of the mercury within the flask, after the melting of the ice, was dried and weighed.

These various weighings afforded data for two independent computations of the density of the ice :

1. From the total mercury necessary to entirely fill the flask ; that necessary to complete the filling after the formation of the ice mantle and that necessary to complete the filling after the ice mantle had been melted.

2. From the direct weighing of the ice mantle and the weight of the mercury which was drawn into the flask when the ice melted.

The first computation which was affected by whatever deformation of the walls may have occurred, gave for the density of ice 0.92154 ; the latter gave 0.91631.

I cite this experiment to show how important the error due to the change of volume caused by filling an ice-calorimeter or a dilatometer with mercury may become.

To avoid altogether the error of the deformation of the calorimeter I adopted in subsequent experiments the method of Brunner, which consists in weighings in air and in a liquid incapable of dissolving ice. The liquid selected was refined petroleum. A sensitive balance was mounted in an unheated room, where the temperature of the air ranged from — 3° to 0° during the course of the experiments. A beaker of the petroleum which had been exposed to cold until it had reached the temperature of the room was placed upon a support within the case of the balance. Care was taken that the set of weights should likewise have been exposed to cold for a time sufficient to give them very closely indeed the same temperature as the balance and its surroundings. The density of the petroleum was obtained by weighing a piece of glass submerged within it. The density of this glass compared with water at 0° was 2.5111 at 20° and its cubic coefficient between — 2° and + 20° was .000025.

The method of weighing in air and petroleum was applied to specimens of the following varieties of ice :

1. Ice mantles frozen around a tube similar in form to the inner tube of the ice calorimeter by the use of a freezing mixture of carbon dioxide and ether.

2. Natural ice in the form of icicles (quite free from air).

3. Natural ice cut about one year before from Dwyer's pond (nearly free from air).

4. Natural ice newly cut from University reservoir (nearly free from air).

The supply of air-free natural ice was obtained as follows: I was walking one day along the ice-bound shore of Cayuga Lake when I noticed icicles of extraordinary clearness hanging from the rocks of a cliff. These icicles, which were formed by the slow trickling out of spring water from the crevices of the rock, were entirely free from air bubbles, and possessed a flawless transparency which reminded one of quartz. I brought several pieces home with me and used them in my determinations.

The results of the measurements made by Brunner's method are given in the following table:

TABLE II.

Redeterminations of the Sp. Gr. of ice by the method of weighing in petroleum.

Kind of Ice.	Temperature of the Weighings.	Density at zero in terms of that of water at zero.[3]	Means.
Ice-mantles (CO_2 and ether).	−1.6°	0.91619	0.91615 ±.00009
	−1.5°	0.91590	
	−0.6°	0.91636	
Natural ice[1] (icicles).	−1.9	0.91816	0.91807 ±.00004
	−1.6	0.91801	
Natural ice[2] (pond ice newly cut).	−0.7	0.91804	
Natural ice (pond ice one year old).	−1.8	0.91644	

The results given in Table II. are noteworthy in that they show a value for a density of the ice-mantles agreeing with the mean result deduced from the measurements of Plücker and Geissler, Kopp and Bunsen, for a similar variety of ice, to four places of decimals.

Since the method was an entirely independent one the probability becomes very great that the correct value for the density of ice in the ice calorimeter is 0.9161. It is doubtful whether any signifi-

[1] Pieces taken from two different icicles.

[2] Result corrected for the presence of air by a method described in a subsequent paragraph of this paper.

[3] In correcting the densities for temperature, Plücker and Geissler's value for the coefficient of expansion of ice, 0.00015, was employed.

cance should be attached to the fifth place of decimals in such determinations as these.

The values for natural ice (newly cut), on the other hand, agree to four places with the value given by Brunner (0.9180). It appears, therefore, that the suspicion of large errors of method in the results given both in Tables I. and II. is ungrounded and that the variations are to be ascribed chiefly to differences in the ice employed.

Under the circumstances it seemed especially important to secure a check upon these measurements for which purpose the following entirely different procedure was devised:

An iron box was constructed, the walls of which were about 1 cm. in thickness. This was built up of slabs of cast iron, the faces of which had been planed and polished, so that the interior surface of the box was everywhere smooth and true. The dimensions of this box were approximately as follows: length 20 cm., height, 9.90 cm., depth 9.90 cm. The lid of the box consisted likewise of a plate of cast iron carefully ground to fit the polished edges of the sides, so as to close the box very perfectly. The contents of the box was obtained by over-filling it with mercury, so that upon setting the lid in place some of the liquid would be expelled between the cover and the edges. The remaining mercury which together with the box had been brought to a known temperature, was then weighed and the cubic contents of the box was computed. The operation was carried on at the temperature of the room, namely 22° C. and the volume of the box at zero was computed by use of the coefficients of expansion of the materials. The contents at 0° C. was found to be 1968.28 cc. A rectangular piece of ice was then cut of such size that it could be inserted in the box without undue jamming. It was found necessary, to this end, to allow a clearance of several millimeters on every side, otherwise the ice could not be inserted without chipping the edges and corners of the block. Mercury was now poured into the interstices between the ice and the iron, care being taken to thus exclude all air. When a sufficient quantity had been introduced to float the ice block, so that its upper face was a centimeter or so above the top of the box, the lid was applied and the ice block was crowded back into place. In this way mercury

was displaced from around it and flowed out between the box and the cover, until finally, when the latter was firmly in place, the box contained only ice and mercury. From the weight of this mercury, subsequently weighed, and from the weight of the ice, together with the previously determined cubical contents of the box, the density of the ice block could be computed.

The specimen of ice selected for this purpose was a portion of a large block newly cut from the University reservoir. In order to correct for the presence of air within the ice, the block was subsequently melted under oil and the air thus set free was measured. It was found that the total volume of air was 0.6 cc., a surprisingly small volume when we consider that the volume of the block was 1476.35 cc. The density of the ice thus measured and corrected for temperature and for the presence of air, was found to be 0.91772.

The specimen described in Table II. as "pond ice newly cut" was from the same block, so that we have a direct comparison between the iron-box method and the method of weighing in oil. The two determinations agree, as will be seen, to about three parts in ten thousand.

It had been my intention to make many determinations by means of the large iron box thus carefully prepared, but a sudden rise of the thermometer to temperatures above 0°C., put a stop to all my experiments. The agreement of the two methods was not what had been looked for but it sufficed to confirm the conclusion, already reached, that the density of natural ice (pond ice, river ice, icicles, etc.) exceeds that of the ice mantles of the ice-calorimeter by about two parts in a thousand.

The arrival of warm weather left this research incomplete in several respects. It had been my intention to study the influence of age upon ice mantles and upon pond ice, also to compare ice mantles produced by means of ether and carbon dioxide with those formed more slowly, after the method of Bunsen, by the use of cooled alcohol. Certain preliminary measurements, made by the method described in the opening paragraphs of Part II. of this paper appear to indicate that mantles formed by the use of alcohol as a refrigerant at $-5°$ to $-10°$, are less dense than those frozen by means of ether and CO_2 at $-70°$, by at least *one part in a thou-*

sand. It was likewise found that one of the latter mantles, upon standing twenty-four hours in an ice-bath, appeared to have lost density by nearly the same amount.

This method was discarded on account of its unreliability, so far as absolute values go, and the statements just made need confirmation before being accepted as established; it was found, however, as has already been pointed out, that the measurements made with a given flask or calorimeter agreed very well among themselves. It is significant in this connection that the only measurement of old pond ice (see Table II.), which was made by the method of weighing in petroleum, a method which yielded consistent results in every instance, showed a density of 0.9164. This would seem to indicate, if we may be permitted to draw any conclusion from a single observation, that such ice tends with lapse of time to approach the lower density of ice which has been formed under the conditions which exist in the ice calorimeter. The whole question of the influence of time and of the conditions under which freezing occurs is, however, one which demands further and more extended investigation.

In conclusion it may be said that the results of these experiments indicate that the natural ice used in the form of air-free icicles or in the massive form of ice blocks cut from the surface of a frozen pond, show density at 0° of 0.9180. Artificial ice mantles produced by the use of carbon dioxide and ether, on the other hand, appear to possess a density not far from 0.9161.

It is interesting to compare these results with those given in the table in the first portion of this paper. My value is greater than that obtained by Plücker and Geissler for the artificial ice mantle produced in their dilatometer by three parts in ten thousand. It is smaller than that obtained by Bunsen by six parts in ten thousand. In the case of natural ice the comparison should be made with Brunner's values, since he used river ice in his experiments. It will be seen that my measurements, by the method of weighing in petroleum, give an average which differs from that of Brunner by less than one part in ten thousand.

There can be no doubt, I think, that natural ice obtained from the surface of frozen ponds and rivers and in the slow formation of icicles possess a density greater than that of artificial ice by about

two parts in a thousand. It is interesting to recall that Dufour in discussing his experiments of 1862, pointed out that the specimens of ice which he used might properly be divided into two groups: those frozen in the open air and those frozen in a closed receptacle. The mean density of the former was 0.9182; of the latter 0.9174. The difference was one upon which he did not venture to lay stress; but its existence now seems to be abundantly verified.

PHYSICAL LABORATORY OF CORNELL UNIVERSITY, August, 1898.

ON THE DIELECTRIC CONSTANT AND ELECTRICAL CONDUCTIVITY OF LIQUID AMMONIA.

BY H. M. GOODWIN AND MAURICE DEKAY THOMPSON, JR.

IN a recent article entitled "The Electrolysis and Electrolytic Conductivity of Certain Substances Dissolved in Liquid Ammonia,"[1] Cady has shown that the molecular conductivity of salts dissolved in liquid ammonia at its boiling point under atmospheric pressure, namely — 34°C., is, in general, greater than the corresponding molecular conductivity of the salts in aqueous solutions at 18°C. For example, the molecular conductivity of potassium nitrate was found to be 124 in ammonia, and 114 in water, at a dilution of 100 liters; potassium bromide 181 in ammonia ($v = 135$), and only 117 in water ($v = 128$); silver nitrate 147 in ammonia ($v = 140$), and only 103.3 at a greater dilution ($v = 166$) liters, in water, etc. Mercuric iodide, which is but very slightly dissociated in aqueous solutions, was found to have a molecular conductivity in liquid ammonia of 102 ($v = 150$). It is probable that the high conductivities found are due to the solute and not to the solvent, since pure liquid ammonia has a conductivity of only 71×10^{-7} (Cady).[2]

The natural conclusion to be drawn from these results, which are admittedly only approximate, but which nevertheless serve to fix the order of magnitude of the quantity in question, is, that liquid ammonia possesses a dissociating power in a very high degree. In fact, other things being equal, one would attribute an even greater power to it than to water, if the molecular conductivities given above be compared at equal temperatures by an extrapolation of the values at 18° C. to — 34° C.

It seemed, therefore, of considerable interest to us, in view of the

[1] Jour. Phys. Chem., 1, 707, 1897.

[2] This is undoubtedly the relative conductivity of liquid ammonia and mercury. The author does not state in what unit it is expressed.

relation shown by J. J. Thomson[1] and Nernst[2] to exist between the dissociating power of a solvent and its dielectric constant, to measure the latter for liquid ammonia. As is well known these properties are, so far as comparison has been made with existing data, approximately proportioned, as well illustrated by alcoholic solvents. The very great dissociating power of water corresponds to its abnormally high dielectric constant. Quite recently additional confirmatory evidence of this relation has been furnished by Whetham[3] in his investigation of formic acid solutions. On the other hand the fact observed by Cady that the addition of small quantities of water to liquid ammonia did not sensibly increase its electrical conductivity, would lead one to infer that the dissociating power of the latter, even if large, was probably less than that of the former. In the present state of our knowledge, experiment alone can decide which of these possible inferences is correct.

Determination of the Dielectric Constant.

In selecting a method suitable for measuring the dielectric constant of such a substance as liquid ammonia, its rapid evaporation and particularly its comparatively high electrical conductivity were deciding factors. We chose as most convenient and best suited for the investigation the method elaborated and used with such success by Drude.[4] We were so fortunate as to have at our disposal Drude's complete apparatus as fully described in the Zeitschrift für Physikalische Chemie, and after a thorough trial of the apparatus and method it is a pleasure for us to second all that the author has claimed for it in regard to convenience and rapidity of working. For a description of the method and procedure reference must be made to the above mentioned article. Only in the following particulars did we find it advantageous to make any change. In order to ensure a minimum of flickering of the discharge in the vacuum tube, and to obtain thereby a corresponding increase in accuracy of setting, we found it expedient to replace the brass spheres and point forming

[1] Phil. Mag., 36, 320, 1893.
[2] Zeit. für Phys. Chem., 13, 531, 1893.
[3] Phil. Mag., 44, 1, 1897.
[4] Zeit. für Phys. Chem., 23, 267, 1897.

the sparking gaps in the primary circuit, with solid platinum. The increased expense was more than compensated for by the durability and excellent results obtained. We would suggest as the best method of securing uniformity of sparking, that the primary of the induction coil and storage cells be connected in series with a rotary brake driven at any desired speed by an electromotor. To prevent excessive sparking at the brake itself, it should be run under water. By this means a very regular succession of sparks is produced in the exciter, the number of which can be varied at will independent of the spark gap.

The first method of measuring the dielectric constant described by Drude, namely, of measuring the wave-length of electrical waves in two parallel wires, surrounded first by air and then by the liquid to be investigated, was open to the serious objection of necessitating the use of large quantities of pure ammonia, as least 200 cc. for each experiment, and working over an open trough of boiling liquid ammonia. The "second method," although less accurate, was therefore chosen. This consisted in introducing into the secondary circuit a small condenser containing the liquid under investigation and noting the length of the circuit for maximum resonance. The settings depended of course on the form or capacity of the cell. The apparatus was calibrated by determining the settings for maximum resonance with the cell filled with various liquids of known dielectric constants. We used benzene, aceton and water as recommended by Drude, their dielectric constants being 2.26, 20.5 and 80.9 respectively at 19°C. For immediate values mixtures of these liquids were used, Drude having determined directly the value of the dielectric current for a number of mixtures of benzene and aceton, and aceton and water, together with their temperature coefficients.

The form of cell used is shown in Fig. 1. It is a small vacuum test-tube in which the exhaustion was carried to a Crookes vacuum. The projecting platinum terminals made contact with the parallel wires of the secondary circuit by means of mercury globules resting in cups sunk in a vulcanite ring support which held the cell. The contents of the cell was 3.5 cc. and the insulation for heat was so excellent that the liquid ammonia remained over the electrodes twenty minutes to half an hour. The height of the ammonia above

the electrodes was of little importance so long as they were well immersed. For calibration purposes we use Kahlbaum's sodium distilled benzene purified by crystallization and then distilled over sodium. Its boiling point was $80°.39 \pm .02$. Aceton was obtained by distilling a sample of Kahlbaum's disulphate preparation over calcium chloride. A perfectly colorless product, distilling at $55°.84 \pm .02$ under 75.789 cm. pressure was obtained. The water used had a specific conductivity of 4.3×10^{-6}.

Fig. 1.

The setting for maximum resonance was determined with these three liquids, as well as mixtures of the two latter, and a calibration plot constructed, with values of the dielectric constants corrected for temperature as ordinates and corresponding cell settings as abscissæ. For the mixtures the dielectric constant was interpolated from Drude's table. From the plot thus obtained the dielectric constant of any substance could be at once determined when the setting for maximum resonance was known. It should be added that no appreciable absorption, so characteristic of compounds containing hydroxyl groups, was noticed in these measurements, the position for maximum resonance being nearly as sharply defined as in the case of benzene or aceton.

Several different preparations of liquid ammonia were used: *First*, ordinary commercial liquid ammonia, used for refrigerating purposes; *second*, a preparation obtained by one distillation of the above; both of these upon evaporating left a slight greasy residue, probably coming from the connections of the iron cylinders used for holding and condensing the ammonia; *third*, a chemically pure product obtained by heating a mixture of sublimed ammonium chloride and lime, and condensing the ammonia gas after it had been thoroughly dried by soda lime in a vessel immersed in a mixture of solid carbonic acid and ether; *fourth*, a product obtained by distilling under pressure commercial liquid ammonia over large excess of lime, in the glass apparatus to be described later. The last two products left no residue on evaporation.

In Table I. are given the corrected results on ammonia, each result being the mean of from sixteen to twenty-five settings. The

temperature was within a degree of $-34°$ C., the boiling point of liquid ammonia at atmospheric pressure.

TABLE I.

DIELECTRIC CONSTANT OF LIQUID AMMONIA.

Preparation.	D. C. at $-34°$ C.
Commercial product	21
Prepared from $Ca(OH)_2$ and NH_4Cl	23
Distilled and dehydrated from commercial product	22
" " " " " "	21.5

From this table it appears that the dielectric constant of liquid ammonia is about 22, a value of the order of magnitude of the dielectric constant of ethyl alcohol and of aceton, but not approaching that of water (about 80). To be sure the liquids are not compared at the same temperature, but were this the case the difference in the values would be even greater, for Abegg[1] has shown that the dielectric constant of water, alcohol and other liquids has a large negative temperature coefficient. Drude's results on benzene, aceton and water obtained by an entirely independent method, also confirm this conclusion. The results of Dewar and Flemming which indicate large positive temperature coefficients are, as pointed out by Abegg,[2] probably erroneous owing to an error of method.

The question naturally arises as to the conditions of temperature which should be chosen for making a comparison of the two properties in question. Should the comparison be made at the same temperature, or at the respective boiling points of the solvents, or at their corresponding temperatures? It seems probable from the experimental data at our disposal, that the dissociating power of a solvent and its dielectric constant are very unequally affected by temperature. The agreement between dissociation values determined by freezing point measurements at 0° C. and conductivity measurements at 25° as well as values obtained by the latter method at temperatures varying from about 0° to 50°[3] indicates that the

[1] Wied. Ann., 60, 54, 1897.
[2] Wied. Ann., 62, 256, 1897.
[3] Jahn, Zeit. Phys. Chem., 16, 72, 1895; Euler, Zeit. Phys. Chem., 21, 257, 1896.

dissociating power of a solvent does not vary appreciably, or only slightly with the temperature. On the other hand, the dielectric constant increases very rapidly with falling temperatures, as pointed out by the authors mentioned above. Abegg has remarked that future experiments may show that solvents having a low dissociating power at ordinary temperatures may, at very low temperatures, possess this property in a high degree.

Assuming then that the electrolytic dissociation does not greatly vary with the temperature, we find that comparing the dielectric constant of liquid ammonia and water at the same temperature, say $-34°$, the difference in their values becomes still greater. If, on the other hand, we compare the dielectric constant at the respective boiling points under atmospheric pressure of the solvents in question, that is 100° for water and $-34°$ for ammonia, we obtain by means of Abegg's formula,[1] which for water takes the form,

$$\text{dielectric constant} = 372\,\varepsilon^{-\frac{T}{190}},$$

T being the absolute temperature, the value 52.2, still about twice that of ammonia.

As to the exact causes which determine the dissociation of a substance we are still much in the dark. Nernst[2] pointed out that there are probably at least two at work: the first an electrostatic attraction of the ions, dependent directly on the dielectric constant of the solvent, and the second, a repulsion, possibly of electrodynamic origin, concerning the nature of which little is known. It would seem, however, that the effect of temperature on these two forces must be approximately equal and opposite in a large number of cases.

An increase of temperature would diminish the D. C. of the solvent and thereby increase the force of attraction between the ions, *i. e.*, tend to diminish the dissociation, while on the other hand the electrodynamic forces tending to separate the ions would be effected in the reverse manner. Only when these second forces are equal for different solvents is proportionality between the D. C. and electrolytic dissociation to be expected.

[1] Wied. Ann., 60, 58, 1897.
[2] Zeit. für Phys. Chem., 13, 531, 1893.

In view of the results obtained on the dielectric constant of liquid ammonia, we thought it desirable to confirm certain experiments on the electrical conductivity of liquid ammonia solutions. Below is a brief description of the apparatus and procedure which we found necessary to devise in order to obtain satisfactory results with this somewhat troublesome solvent.

ELECTRICAL CONDUCTIVITY OF LIQUID AMMONIA.

Our first experiments were made in the manner described by Cady, namely by the usual Kohlrausch method, using a vacuum test-tube cell provided with two parallel vertical electrodes. This form of cell was found, however, to be very unsatisfactory for the following reasons: First, the liquid boiled from the electrodes, thus causing a very doubtful minimum in the telephone; second, the conductivity continually increased owing to the evaporation of the solvent; third, the temperature had to be assumed that of boiling

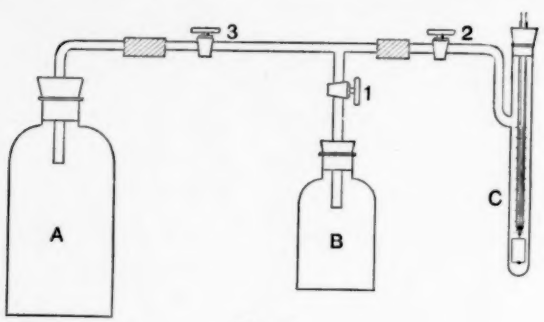

Fig. 2.

liquid ammonia, and consequently, fourth, the measurement of the conductivity at different temperatures was impossible. We decided, therefore, to carry out our measurements in a closed cell under pressure. After much experimenting the following arrangement was found to work very satisfactorily: Liquid ammonia was run into a stout glass bottle *A* of 250 cc. capacity, containing a large excess of unslaked lime. This, the distilling bottle, was then connected with *B*, a stout bottle of 150 cc. capacity, and with *C* the conductivity cell, a larger sketch of which is shown in Fig. 3.

The latter was made from a burette so that the volume of the

liquid in the cell could be read off at any time. The true volume corresponding to any reading was determined from a calibration plot obtained by running in from a second burette known volumes of liquid, and observing the reading when the electrodes were in place. The stopcocks and connecting tubes were of stout glass, and the rubber connections, which were securely wired on, of one-quarter-inch pressure tubing. The rubber stoppers which closed the bottles had also to be wired on in each case. The glass tube through which the leads of the electrodes passed was securely held by means of sealing wax in a piece of vulcanite, which in turn fitted tightly in a soft-rubber stopper. The electrodes were platinized with the solution recommended by Lummer and Kurlbaum and gave an excellent minimum.

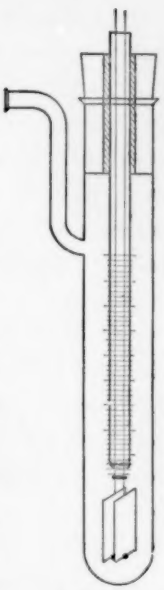

Fig. 3.

The apparatus was set up as follows: The electrodes were first securely wired in place and the parts of the apparatus connected together with the exception of the distilling flask *A*. The stopcocks were then all opened and *B* and *C* exhausted by an air pump. Stopcocks 1 and 2 were then closed and 3 opened by withdrawing the stopper completely in order to allow the ammonia to escape freely while the distilling flask *A* was connected to the apparatus. When this was effected (best in the open air), *B* and *C* were placed in a freezing mixture of salt and ice, and as soon as they had assumed the temperature of the bath, cock 3 was replaced and 2 opened. The ammonia then distilled over, perfectly anhydrous and pure into the measuring cell. When the desired amount had been collected, 2 was closed and 1 opened to prevent an excessive pressure from being generated in the apparatus. The conductivity of the sample collected in *C* was then measured in the ordinary way. A duplicate measurement could be made without disconnecting the apparatus by distilling off ammonia from *C* over into *B* and then collecting as before a new sample in *B* from *A*. When only one sample was desired, the cell was disconnected completely from the distilling apparatus at the rubber joint in front of 2 as soon as sufficient ammonia had distilled over.

The procedure for measuring solutions was the same, except that a small amount of the solid salt was weighed out into the cell at the start. The ammonia was allowed to distill over until the cell was nearly full, thus giving at the start the dilutest solution obtainable with the amount of salt used. The cell was then disconnected from the distilling apparatus. The temperature was obtained by an alcohol thermometer, bound on the outside of the cell, the bulb being placed opposite the electrodes. It was anticipated that some difficulty would be met with in obtaining a well stirred homogeneous solution, but by the above described procedure the difficulty was easily overcome in the following way: The stopcock 2, which should turn readily, was slowly opened until the pressure was relieved just enough to cause the liquid to boil gently. A platinum point sealed in the very bottom of the cell facilitates regular boiling. The rising bubbles of vapor effect a thorough stirring of the whole liquid as indicated by constant settings on the bridge when the solution has been allowed to reëstablish the original temperature of the surrounding bath. As soon as constant readings were obtained, both on the thermometer and bridge, the volume of the solution was read off, which completed all data necessary for computing the molecular conductivity at that dilution. This procedure did away with the necessity of using a stirrer, which might of course be so devised as to be actuated by an electromagnet from the outside. To obtain a more concentrated solution the stopcock was then again opened and a cubic centimeter or more of ammonia allowed to boil off, and the above measurements repeated. This procedure was continued unil the remaining solution barely covered the electrodes. The above method has been found to work very satisfactorily, all the difficulties on page 44 being avoided.

The temperature coefficient was determined by placing the cell in a large test-tube containing a mixture of solid carbonic acid and ether, the temperature of which was allowed to rise very slowly. Simultaneous readings of temperature and conductivity were taken and plots then constructed, showing the change of conductivity with temperature, from which the temperature coefficient was obtained graphically. It is quite likely in these measurements that the temperature of the solution lagged somewhat behind that of the bath. But if the lag were uniform, no great error is thereby introduced in the rate of the change.

TABLE II.

SPECIFIC CONDUCTIVITY OF LIQUID AMMONIA. TEMPERATURE COEFFICIENT.

Temp.	K × 10⁴	Temp.	K × 10⁴	Temp.	K × 10⁴	Temp.	K × 10⁴
−17.7	1.627	−25.0	1.539	−27.0	1.485	−29.5	1.392
−16.5	1.648	−21.0	1.577	−25.0	1.502	−26.8	1.415
−15.9	1.654	−20.0	1.589	−23.4	1.520	−25.2	1.432
−15.0	1.661	−19.1	1.596	−23.1	1.522	−24.0	1.443
−14.1	1.677	−18.1	1.608	−21.0	1.539	−22.0	1.467
−13.0	1.688	−17.2	1.611	−18.5	1.564	−20.0	1.485
				−17.5	1.577		
				−16.0	1.583		
				−15.0	1.589		
Mean		Mean		Mean		Mean	
$10^4 \times \frac{\Delta K}{\Delta t} = 0.012$		$10^4 \times \frac{\Delta K}{\Delta t} = 0.0098$		$10^4 \times \frac{\Delta K}{\Delta t} = .0093$		$10^4 \times \frac{\Delta K}{\Delta t} = .012$	

Table II. contains the results of several series of measurements of the specific conductivity of pure liquid ammonia at different temperatures. The mean results of the four series gives $\frac{\Delta K}{\Delta t} = 0.011 \times 10^{-4}$ for temperatures between $-30°$ and $-12°$ C. As the value of the specific conductivity of liquid ammonia may be taken in round numbers to be about 1.6×10^{-4} it is seen that the conductivity increases about 0.7 % per degree. The value of the coefficient of water has been found to be 2.5 % per degree. Ammonia has, therefore, an unusually small temperature coefficient compared with other electrolytes.

The molecular conductivity of silver nitrate dissolved in liquid ammonia was next determined and the results obtained confirm those published by Cady that is the molecular conductivity in ammonia was found to be greater than in water.

TABLE III.

Molecular Conductivity of Silver Nitrate in Liquid Ammonia.

Volume in Liters.	Molecular Conductivity of $AgNO_3$.	Temperature.	Volume in Liters.	Molecular Conductivity of $AgNO_3$.	Temperature.
110	180	−16.4° C.	54	153	−14.5
94	176	−16.3	30	134	−12.2
78.9	171	−16.2	140	147	−34. (Cady)
64	163	−15.2			

In Table III. are given the results obtained at various dilutions. It will be noticed that the molecular conductivity increases with the dilution as in the case of aqueous solutions of inorganic salts. A determination of the $\mu\infty$ values would have been of much interest, but on account of the inaccuracy attendant on weighing out excessively small quantities of salt, it seemed unfeasible to attempt to carry out such measurements with the size of apparatus which we used.

The values in this table indicate, however, that the rate of increase of the molecular conductivity unmistakably diminishes; hence it is probable that the molecular conductivity approaches a limiting value somewhat beyond $V = 100$ liters. This would point to a high degree of dissociation of the dissolved salt, as has been assumed in this paper. The fact that mercuric salts conduct well in liquid ammonia also confirms this assumption. As the corresponding values of the molecular conductivity in aqueous solutions are considerably less, it is not improbable also that the velocities of migration of the ions in liquid ammonia are greater than in water. An investigation of this difficult problem would be of much interest. It would also be of interest to determine whether liquid ammonia renders conducting other substances than mercuric salts which are little if any dissociated in water, for example, certain cadmium salts, and certain weak bases.

ROGERS LABORATORY OF PHYSICS
MASSACHUSETTS INSTITUTE OF TECHNOLOGY.

SOME EXPERIMENTS IN MOLECULAR CONTACT.

By James S. Stevens.

IN working with an interferometer it was observed that one could slide the brass plate which carries the movable mirror along the bed upon which it lies so that the fringes would suffer displacement for a small but definite amount, and that when the plate was released they would return to their original position. The idea occurred to me that it might be possible to measure these displacements for given weights applied to move the mirror-plate, and ascertain if they followed any law, and what relation these experiments bore to the ordinary experiments for finding the coefficients of friction.

For this purpose I had constructed four plates — of brass, copper, steel and cast iron, — whose dimensions were 8 × 5 × 1 cm. One side and one edge of each were smoothly polished so that the friction was reduced to a minimum. The following table shows the weights, loads and coefficients of friction of repose for each case:

TABLE I.

Coefficients of friction—Ordinary method.

Arrangement.	Weight of plate moved.	Load.	Coefficient.	Coefficient for edge.
Copper on brass	272.7g	60.1g	0.22	0.21
Brass on copper	255.0	52.0	0.20	0.17
Steel on iron	241.0	61.0	0.25	0.19
Iron on steel	220.5	37.0	0.16	0.14
Copper on steel	232.7	46.0	0.19	0.25
Copper on iron	232.7	41.0	0.17	0.17
Brass on steel	255.0	41.0	0.16	0.27
Brass on iron	255.0	37.0	0.15	0.18
Steel on copper	241.0	46.0	0.19	0.18
Iron on copper	220.5	42.5	0.19	0.18
Steel on brass	241.0	31.0	0.13	0.18
Iron on brass	220.5	36.0	0.16	0.12

These determinations were made chiefly for reference and were probably in error by as much as two per cent. The apparent inconsistencies may be explained by inequalities in polish.

The next step was to fasten the moving mirror, *D*, with wax on one end of the upper plate, *A*, — the one to be tested, and then find the fringes. In the end of the plate opposite from the mirror were inserted two screw hooks and to these was fastened a scale

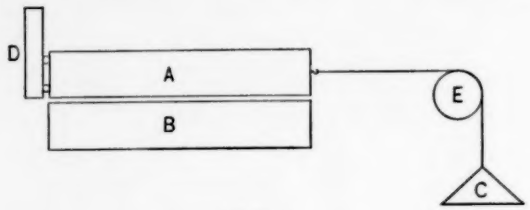

Fig. 1.

pan, *C*, by means of the cord passing over the pulley *E*. The cord and pulley weighed 6 g. and the mirror 5.9 g. Weights were placed in the pan and the resulting displacements of the fringes observed.

The table below contains the results of the various tests :

TABLE II.

Measurements by displacement of fringes.

Load.	Copper on Brass.	Brass on Copper.	Steel on Iron.	Iron on Steel.	Copper on Steel.	Copper on Iron.
11g	0.00λ	0.00λ	0.00λ	0.03λ	0.03λ	0.00λ
16	0.03	0.00	0.03	0.08	0.04	0.03
26	0.05	0.05	0.08	0.10	0.06	0.09
36	0.10	0.08	0.10	0.15	0.11	0.11
46	0.10	0.08	*	*	0.13	0.15
56	*	*			*	0.18
66						*
Load.	**Brass on Steel.**	**Brass on Iron.**	**Steel on Copper.**	**Iron on Copper.**	**Steel on Brass.**	**Iron on Brass.**
11g	0.00λ	0.00λ	0.03λ	0.00λ	0.00λ	0.03λ
16	0.05	0.03	0.05	0.03	0.03	0.05
26	0.08	0.08	0.08	0.08	0.05	0.08
36	0.10	0.13	*	0.10	0.08	0.10
46	0.13	*		*	0.10	0.13
56	*				0.13	*
66					*	

Table III. shows the results of two experiments with the plate on its edge ; Table IV. where the rough surfaces of copper and brass

were used; Table V. where an additional weight of 235.1 g. was placed on the brass plate while it moved on the copper; in Table VI. steel was moved on steel, first on its side and then on its edge.

TABLE III.

Plates on Edge.

Load.	Copper on Brass.	Steel on Iron.	Load.	Copper on Brass.	Steel on Iron.
11g	0.00λ	0.00λ	36g	0.13λ	0.08λ
16	0.03	0.03	46	*	0.13
26	0.08	0.05	56		*

TABLE IV.

Rough Sides.

Load.	Copper on Brass.	Load.	Copper on Brass.
16g	0.00λ	56g	0.08λ
26	0.03	76	0.23
36	0.05	96	*

TABLE V.

Weight added (235.1g).

Load.	Brass on Copper.	Load.	Brass on Copper.
16g	0.03λ	46g	0.10λ
26	0.05	56	0.15
36	0.10		

TABLE VI.

Steel on Steel.

Load.	On Side.	On Edge.	Load.	On Side.	On Edge.
11g	0.00λ	0.00λ	46	0.15λ	0.15λ
16	0.03	0.03	56	0.15	*
26	0.05	0.05	66	0.20	
36	0.10	0.10	76	0.25	
			86	*	

Coefficient by ordinary method 0.23.

An inspection of these tables shows that in most cases 11 g. and in every case 16 g. was sufficient to cause motion in the upper plate. In every case except those marked * the fringes returned to their original position when the weight was removed.

The accompanying plots are based upon the data in Table II. They are approximately straight lines and departures therefrom would seem to be due to inaccuracies in reading and inability to make readings for loads differing by less than 5 g. The motion of the fringes was measured with reference to a vertical wire placed in

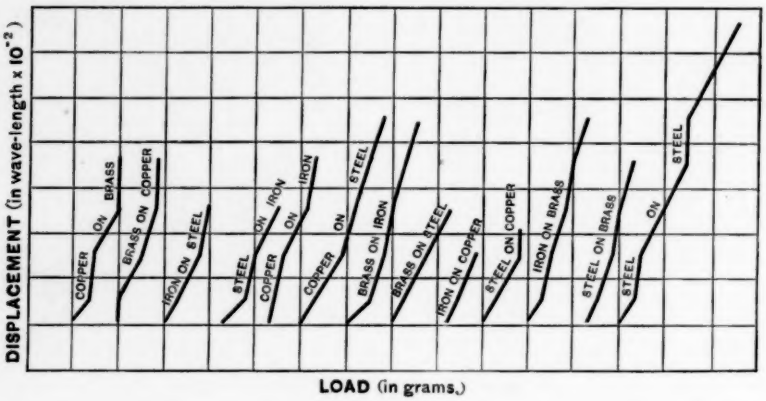

Fig. 2.

front of the moving mirror. Motions through a dark or yellow space (sodium light being used as a source) is equivalent to 0.25 wave-lengths. By various methods this motion may be read accurately to tenths of a space or 0.025 of a wave-length. In each case the number set down in the table represents a mean of several trials so that it is safe to estimate that the readings are not in error by more than 0.01λ. Probably the error is less than this quantity.

In conclusion it may be said:

1. When a proper force is applied there is a motion of one surface over another which takes place before the body actually begins to slide.

2. This motion, unlike that produced when friction is overcome, is imperceptible under ordinary conditions, and the plate returns to its first position when the force is removed.

3. These minute distances passed over by the plate are nearly (probably exactly) proportional to the force applied to produce the motion.

4. This law seems to hold for the case where a plate slides on its

edge, for rough surfaces as well as smooth, for dissimilar surfaces as well as similar, and when an extra weight is placed on the moving plate.

5. As a possible explanation it is suggested that when a force is applied which is not sufficient to produce a complete rupture between the molecules in contact, there is introduced an additional stress which pulls them into their ordinary position when its cause (the force applied) is removed.

PHYSICAL LABORATORY, UNIVERSITY OF MAINE.

A PROPOSED TIDAL ANALYZER.[1]

BY ROLLIN A. HARRIS.

INTRODUCTORY.

SUPPOSE the heights of the surface of the sea, as shown by a tide-staff or self-registering gauge, to be tabulated at equal increments of time, say at each solar hour. If for many days we combine the heights belonging to each solar hour, there will be obtained 24 partial sums or means. These if plotted or analyzed will show the magnitude and position of the daily fluctuation of the water's surface; *i. e.*, they will represent the mean solar tide. There will, in this case, be two complete oscillations in 24 hours, and there will usually appear to be almost identical. But it is known that whatever may be the shape of the periodic curve defined by these 24 means, it can always be represented by a Fourier series—the period of the slowest term being a day, the periods of the other terms being a half day, quarter day, etc. The amplitudes and epochs of the terms making up this daily fluctuation of sea-level, are determined by a well-known analytical process styled "harmonic analysis."

In like manner, the fluctuations of the sea might be tabulated in lunar time and the 24 sums or means would show the lunar daily fluctuation in the surface of the sea. Terms having periods of a half lunar day and its submultiples are brought to light, as before, by the process of harmonic analysis.

The inequalities in the apparent diurnal motions of sun and moon (due to ellipticity of orbit, declination, etc.) give rise to additional oscillations of the sea having periods of approximately a day or a half day.

In all cases the periods of the oscillations are known in advance from astronomical considerations. The oscillations or component tides are referred to assumed astronomical causes or arguments when the analysis is made. For instance, the principal lunar tide is

[1] Read before Section A, Boston meeting of the American Association for the Advancement of Science, August, 1898.

referred to the time of transit of the mean moon across the local meridian.

One of the great advantages of the harmonic treatment of tides over any less methodical process is the fact that it readily lends itself to mechanical aids, thus saving an enormous amount of brain energy.

At first this saving was realized only in part. Early workers had in mind either tables for showing the proper distribution of hourly heights or blank forms into which the heights were to be copied for each kind of component summation. Nearly all of the results published in the Proceedings of the Royal Society and the reports of the Indian Survey were obtained by this rather laborious mode of procedure.

Stencils, etc.

Comparatively few analyses were thus made by the U. S. Coast Survey; for in the year 1885 one of its members, Mr. L. P. Shidy found that this copying and recopying was wholly unnecessary. He in that year devised sets of perforated sheets, known as stencils, which have since been in constant use in the Survey to the exclusion of all other processes. These reduce the chief part of the work of making an analysis to summing in the ways indicated by the stencil sheets, in other words saving all copying.

For each kind of summation a set of sheets is required. As a matter of convenience, the period of time covered by each pair of sheets is taken as one week, and so about 53 such pairs of sheets constitute a set suited to a series of observations one year in extent.

The hourly heights of the sea are tabulated in mean solar time; the correspondence between solar and lunar hours, both of which are assumed to be zero at the beginning of the series, is indicated by the *M*, or principal lunar, stencil. Similarly, for other kinds of hours. Suppose a stencil sheet to be applied to a page of tabulated hourly heights; the openings then show certain ones of these heights beneath.

A broken line bearing a particular number (from 0 to 23) shows that the heights appearing through the holes thus connected are to be added together. When the whole series has thus been summed

by applying the whole set to stencil sheets, the twenty-four partial sums are then analyzed upon a form adapted to the purpose.

A full description of the construction and use of the stencils will be found in the report of the Survey for 1897.

Some years ago, but subsequently to 1885, Dr. Börgen hit upon essentially the same idea; but his usage of transparent instead of perforated sheets must detract somewhat from the ease of reading the heights beneath. More recently Professor G. H. Darwin devised a set of movable scales upon which the hourly heights for a particular year or portion of a year are copied; these scales are then set for various kinds of summation in accordance with a printed form below them. Practically the same result would be obtained from them as from the stencils, but in a less convenient manner. The cost of such an apparatus would be many times that of the simple perforated sheets.

THE THOMSON HARMONIC ANALYZER.

This analyzer might, at first sight, seem well adapted to tidal work; but a little reflection will show that numerous obstacles must arise to its practical application. For instance, the position of the tide curve is not usually permanently fixed with reference to a horizontal base-line upon the tide-sheet; the working scale of the tide gauge may not remain constant; the time scale is seldom uniform or constant. It is an advantage for future work or verification to have all hourly heights carefully tabulated and reduced to a fixed plane of reference; consequently there is no advantage in having a machine treat the curve directly. In other words, nothing is lost in tabulating the hourly heights. The process of making the analysis when the partial sums are once obtained is brief and simple, so that there is little call for mechanical aid in this direction.

About the only labor-saving machinery which still remains to be actually applied to the harmonic treatment of tides seems, to the writer, to consist of a suitable sorting and adding contrivance. The possibility of such a machine grows naturally out of the sorting process as carried on by means of the stencils.

To this end the writer has constructed an illustrative wooden model and considered somewhat carefully the requirements of the case.

A Proposed Machine for Obtaining Component Sums.

Instead of a stencil sheet for pointing out how the hourly heights are to be distributed in making up the twenty-four or twenty-five partial sums belonging to each independent summation, imagine a perforated cylinder into which needle points can drop at the proper times and thereby control an adding or registering apparatus placed above. In the illustrative model (Fig. 1) the numbers (hourly heights) are entered into the machine by revolving the large wheel through a certain portion of its circumference. This causes the cog-wheels (constituting the registering apparatus) to revolve a like amount as would be shown by the numbering of the cogs. The controlling cylinder (not shown in the figure) lies below the row of needles. A needle upon entering a hole in the cylinder causes the projecting screw near the other end of the lever to engage in the teeth of the wheel above and prevent its rotation, while the other wheels are free to rotate through the distance representing the number (hourly height) being entered. By pulling a pin between the knob and center of the large wheel, the latter is free to revolve back to its initial position while all the cog-wheels remain stationary. As it approaches its initial position, the pins are automatically hoisted from the surface of the cylinder and the cylinder moved forward one notch by means of a rack and pinion—the latter being upon the same shaft as is the toothed wheel which is scarcely visible in the nearest corner of the photograph. The large wheel rotated again enters another number (hourly height) and so on.

This is simply for illustration; only the *M*-summation is provided for and only small numbers can be entered. A machine for practical purposes, while just as easily operated, would sum for all components simultaneously and may be described as follows:

Let there be as many cylinders, each say twenty-six inches long and ten inches in diameter, as there are independent summations to be made. They are represented in Fig. 2 by the lowest of the three rows of circles. Each cylinder will represent the stencil of a single component for, say, 370 days.[1]

[1] In the figure, double cylinders each about fifty inches long, instead of twenty-six inches, and representing two components, are used in order to give better dimensions to the machine.

The circumference of each cylinder should be divided into 370 equal parts, each division fixing a line or element which represents a day. All cylinders are supposed to have a common movement in the direction of their axes, an inch in extent, in order to bring

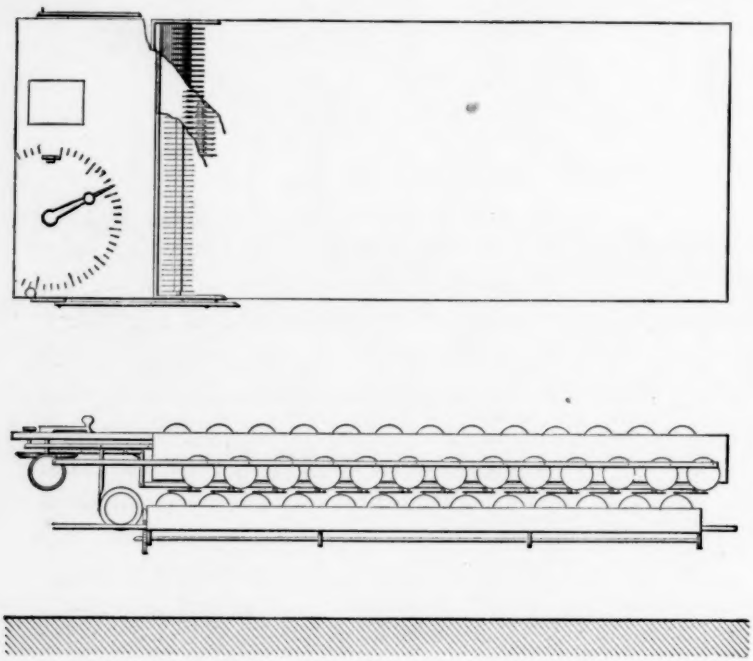

Fig. 2.

the holes into their proper positions for the various hours of the day. Each day line contains twenty-four holes in the surface of the cylinder determined by the correspondence between solar and component hours for the day in question, the small movement just referred to having been taken into account.

The recording or adding apparatus for each kind of summation consists of two series of toothed wheels, all wheels of a series being upon a common shaft; the number of teeth upon each wheel of the first series may be taken as 300, and of the second series 299. The number of wheels in each series is twenty-five—one per each component hour, and one for those few hourly heights which are used only in checking the sums of the twenty-four partial sums. The

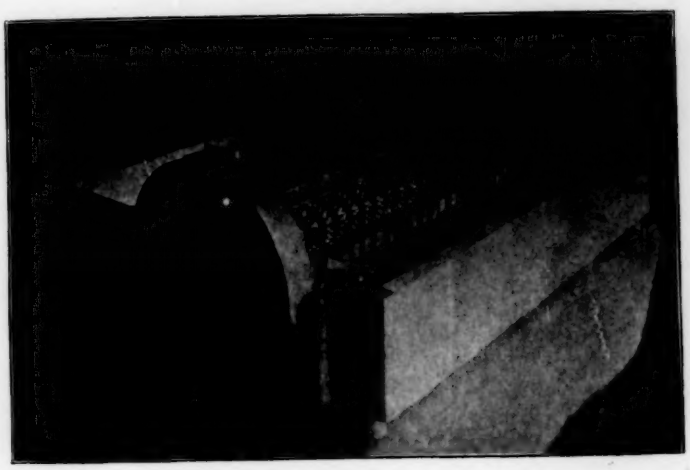

Fig. 1.

HARRIS: A Proposed Tidal Analyzer.

number of complete revolutions made by the 300-tooth wheels can be found by subtracting the readings of the 300-tooth wheels from the readings of the 299-tooth wheels. The parts of revolutions are, of course, the direct readings.

The cylinders are placed side-by-side, in a horizontal frame, all axes being parallel. This frame should have a small rack-and-pinion movement in the direction of the axes of the cylinders. All cylinders are made to rotate together, one notch for each day, by means of a rack and spur wheels.

Above these cylinders, or above the intervening spaces, the two sets of toothed wheels serving as counters are mounted. They are represented in the figure by the middle and highest rows of circles. Between the two sets are small idle pinions not shown in the figure; these make it possible to use thin material for the wheels themselves. The shafts bearing the 300-tooth wheels can all be rotated the same amount by means of parallel rods and cranks. The operator is supposed to impart motion to the mechanism by means of a crank at one end of the stationary frame-work. This carries a pointer which moving over a graduated dial indicates the amount of its rotation and those 300-tooth wheels which are not held fast by the levers about to be mentioned; that is, it indicates what number or hourly height is being entered. The crank can be released and returned to its initial position without causing any of the shafts to rotate. Of course a set of keys could replace the somewhat primitive crank arrangement.

The cylinders control the 300-tooth wheels by means of levers, one for each wheel. The twenty-five levers for each component are upon a common axis parallel to the axis of the cylinder. At one end of the lever is a pin or projection for entering the holes in the surface of the cylinder while at the other end is a sharp edge extending upward for engaging the wheel above, thereby stopping its motion. Since the preventing of a wheel from rotation with its shaft must give rise to friction and wear, it is best to stop but one out of each twenty-five, rather than to stop twenty-four of them. This involves no extra work on the part of the operator except the subtracting of the final machine readings from a constant number—the grand total of all hourly heights.

For each succeeding day, the cylinders are all turned forward one notch; and for each succeeding hour of the day, they are all automatically carried a small but constant amount along the line of their axes. As already intimated, the hourly heights when entered once are to be simultaneously summed in all the kinds of summation required in analysis, thus enabling a person to sum for all components almost as quickly as he now sums for one upon an ordinary adding machine.

U. S. COAST AND GEODETIC SURVEY, WASHINGTON, D. C.

NEW BOOKS.

Die elektrodynamischen Grundgesetze und das eigentliche Elementargesetz. By FRANZ KERNTLER. 8vo, pp. 68. Budapest, Pester Lloyd. 1897.

This little book, or rather pamphlet, is devoted to a discussion of the various mathematical expressions that have been proposed for the mechanical force exerted by one infinitesimal current element upon another. The author looks upon the law of mutual action between two current elements as the foundation of electrodynamics, and therefore considers the form in which this law is stated as of prime importance.

The attention of the reader is first called to the fact that Ampère's expression for the action of one element on another is not the only one that gives results in agreement with experiment. This fact is usually mentioned in the more rigorous treatises on Electricity; Maxwell, for example, devotes a chapter to the subject. But in many books the matter is touched upon only briefly, and the reader is often led to believe that Ampère's expression is the statement of a real and rigorously proven physical law, rather than merely one of an infinite number of expansions, each of which, when integrated around a closed circuit, will give correct results. Kerntler's discussion of the subject brings out this point with great clearness. Indeed from some points of view his arguments are *too* conclusive. For, after convincing us that the law of force between current elements is incapable of experimental derivation, and that the choice of an expression for this force is to be determined merely by its convenience in computation, the author proceeds to advance a new law, more complicated than Ampère's, which he regards as the "*real* law of force."

As Maxwell has pointed out, the only electric currents known to us are such as follow in closed circuits. Even in the case of a circuit containing a condenser we think of the current as being continued through the dielectric in the form of a displacement current. To determine by direct experiment the force exerted by a *part* of one circuit on a *part* of another is therefore from the nature of the case impossible. All that we can *observe* is the action of one closed current upon another; or, in the case of a flexible circuit, the action of a complete closed circuit upon some part of a closed circuit. It is perhaps natural to think of this action as resulting from the action of infinitesimal parts upon each other, and Ampère has shown that if force between two elements ds and ds' is given by the expression

$$\frac{ii'ds\,ds'}{r^2}\left(\cos\omega - \frac{3}{2}\cos\theta\cos\theta'\right)$$

then the total force between two currents, as derived from this expression by suitable integration, is always equal to the force actually observed.[1] But although this expression leads to correct results in all cases where experimental verification is possible, there is not the slightest reason for believing that it corresponds to any real physical law. Ampère's law of force must in fact be looked upon as a solution of the following problem: the force between two complete currents being known, and the assumption being made that this force is the resultant of forces between the individual elements of the circuits, it is required to find the expression for the force between two elements. Some of the peculiarities of such a problem may be brought out by a very commonplace illustration. Let ten men catch hold of one end of a rope and pull, while another ten men catch hold of the other end of the rope and pull in the opposite direction. Let us imagine that some device, say a spring balance, enables the total tension in the rope to be measured. Can we determine from this what force is exerted by each one of the men? No very elaborate argument is needed to show that the two problems are equally indeterminate, and that the conditions can be satisfied by an infinite number of solutions.

In the first few chapters of the book before us the author presents somewhat the same view of the matter as that stated above. Numerous illustrations are given of the simpler expressions for the force between two elements, and the advantages of each, as well as the assumptions involved in its derivation, are briefly discussed. This portion of the book will be found both interesting and instructive reading. It is somewhat easier to follow than the corresponding discussion in Maxwell, and the number of cases considered is greater.

But while the earlier chapters are on the whole to be commended, the same cannot be said of the later portions of the book. In fact, the soundness of the author's reasoning becomes occasionally questionable quite early in the book. Take, for example, the very plausible argument advanced on page 10 regarding the action of one circuit in producing rotation in another. The statement is made that while all of the numerous expressions for the force between elements give identical results in the case of translational forces, they differ greatly when used to compute the forces tending to produce rotation; in the former case the total effect is found by adding the forces on the two halves of the circuit, while in the latter case it depends upon the difference of the two effects. The author proposes to employ this principle in determining which of the various expressions is correct. To show that both the final conclusion and the

[1] In the above expression ω is the angle between ds and ds'; r is the distance between the two elements; and θ and θ' are the angles between the two elements and the line joining them.

original statement are incorrect is a matter of no great difficulty. But the reasoning is very plausible, and to point out the flaw in each step will be found rather troublesome.

In developing the expression which is proposed as a substitute for that of Ampère the author imagines the two current elements to be replaced by their components along the line joining them and in two directions at right angles to this line. Certain mathematically simple assumptions are made regarding the action of these components upon each other, and the resultant force finally comes out in the rather symmetrical form

$$R = \frac{ii' ds ds'}{r^2} \sqrt{1 - \sin^2\theta \sin^2\theta' \sin^2\omega}.$$

I am unable to see that this form of expression presents any advantages in computation; in fact the author makes no such claim. The force R, although the same on each of the two elements, does not act in the line joining them. If the expression is looked upon merely as a mathematical convenience there is no reason why the forces *should* act in the same line: Ampère's expression is in fact the only one in which the force acts in the line joining the elements. But from the physical standpoint there are serious objections to a law that would lead to the continuous rotation of a pair of isolated current elements.

On the whole, in spite of the fact that the author's conclusions will probably not meet with full approval, I think that the book is one that may be read with interest and with profit. ERNEST MERRITT.

Animated Pictures. An exposition of the historical development of chrono-photography, its present scientific applications and future possibilities, and of the methods and apparatus employed in the entertainment of large audiences by means of projecting lanterns to give the appearance of objects in motion. By C. FRANCIS JENKINS. Published by the author, Washington. 1898. Pp. 14 + 118.

The author of this volume had already published an account of his work, in the development of the processes of chrono-photography for the projection of animated pictures, in the little book entitled "Picture Ribbons." The present work, while it deals with the same topic, is not to be regarded as a new edition of that volume. Its contents are, indeed, almost entirely new. The book is divided virtually into three parts: We have first an historical sketch of the development of chrono-photography, beginning with the zoötrope and leading up to the latest devices for the production of animated pictures; then a detailed description of the author's form of projection apparatus, namely, the phantoscope, and of various chrono-photographic cameras. These chapters are followed by detailed descriptions of the development and print-

ing of long films and methods of perforating the ribbon. The volume closes with a chapter of suggestions concerning the handling of machines of the phantoscope type.

The illustrations in Mr. Jenkins' volume are of two classes; cuts, diagrams and photo productions of the instruments which he is describing, and, secondly, a set of full-page plates in half-tone, the connection of which with the text is by no means obvious. The latter, while pleasing in themselves, strike one as out of place. The volume suffers for lack of an index and in many minor particulars one misses the completeness of form and arrangement which would have been ensured had it passed through the hands of some experienced publisher. It contains, however, so many interesting details concerning the art of which it treats that one really overlooks these unessential matters. The author's enthusiasm for his subject and his complete familiarity with it, at first hand, are strongly in evidence throughout the work. E. L. N.

Alternate Currents in Practice. Translated from the French of LOPPÉ & BOUQUET. By FRANCIS J. MOFFETT. Octavo. Pp. 376. London, Whitaker & Co., 1898.

This is a clear and rather comprehensive work, dealing, we might say, with the theory of practice. The work is not a practical one in the ordinary sense of the term. It is free from trade cuts, which so often fill up so-called practical treatises, and the description of insignificant details of particular machines which may, perhaps, be obsolete by the time the book has left the press. Yet the subjects discussed are practical. The book does not contain purely academic discussions and abstract analyses of subjects which have no practical application.

The subjects of alternators, motors, transformers, condensers, distribution and measurement are all covered in this one volume. Exhaustive treatment of any one part of the subject is necessarily impossible, and one may be disappointed upon looking up a particular subject to find it passed over with a brief paragraph which gives him little definite information. For example, in the chapter on "Industrial Measurement of Alternate Currents," the treatment of the measurement of the efficiency of alternators, motors and transformers is all comprised in a single page, and the discussion throughout the whole of the chapter consists chiefly of generalities without sufficient detailed statement to be of assistance to one about to undertake measurements either commercially or in the laboratory. The same criticism may be made of other portions of the book. Yet, in covering the whole subject in one volume of this size, it is necessary to slight some parts, and the authors have done well in bringing together as they have theory, design, experiment, and practice.

F. B.